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MEASUREMENT[1]

By J. J. C. SMART

The word 'measure', it will become apparent, is used in many different, though related, senses. For example, measurement of length is a very fundamental sort of measurement: using a stick as standard, and by explaining what 'equal in length' means by putting sticks side by side and what 'addition of length' means by putting sticks end to end, a man on a desert island could build up and teach a system of measurement of length to a set of scientifically illiterate people. On the other hand, measurement of the charge of an electron is something which could only be done by, and taught to, people in possession of a considerable body of experimental and theoretical physics. Different in another sort of way again is measurement of temperature. There is a sense, I suppose, in which we can say that a temperature of 200° absolute is twice a temperature of 100° absolute, but it is an odd sort of sense. We cannot put temperatures end to end in the sort of way we can put bits of wood end to end. Different again is the sense of 'measurement' used by psychologists, where the only means used to assess test scores is counting, with the results of counting manipulated arithmetically through considerations arising from the theory of probability. We can think of the various uses of the words 'measurement' and 'magnitude' as related by a sort of 'family resemblance' (to use a simile of Wittgenstein's). We can think of them as like parents, sons, and daughters, grandsons and granddaughters. We may also come across cousins, second-cousins, great-uncles and great-aunts, about whom it will be somewhat arbitrary whether we include them in the family or not. (Psychological measurement, for example, is such a distant relation, and hence the inconclusiveness and futility of many discussions of whether psychological measurement is 'really measurement' or not.)

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[1] Based on a paper read at Section J of ANZAAS, Melbourne, 1955.

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Some uses of the word 'measurement', as I have suggested, presuppose others. Let us consider the sort of measurement that presupposes no other sort, the sort of measurement that could be taught to a savage on a desert island. Measurement of length, mass, and periods of time are some (though not all) of the kinds of measurement of this bedrock sort, which N. R. Campbell (whose ideas on measurement I shall make considerable use of) appropriately called 'Fundamental Measurement'.[2] (Though I deviate from Campbell's terminology slightly. For example, what I shall call a 'fundamental defined magnitude' is treated by Campbell as a 'derived magnitude'. This is, I think, because he is insensitive to the distinction between logically necessary and empirical propositions..)

Before teaching a savage the concept of length or mass it would be necessary to teach him to count, and so if we regard counting as measurement then counting is the most fundamental sort of measurement of all. (Though 'count', like 'measure', is a polymorphous concept. Compare 'count the apples' with 'count the roots of the equation'.) Let us, however, assume that the savage has already been taught to count and to do elementary arithmetic.

When we say that a property is a fundamental magnitude we mean that there is a procedure of fundamental measurement which determines it. What criteria must a property satisfy to be of this sort? First of all, the property must be such that it enables us to put things possessing it into a unique *order*. Thus, to use a common example, the relation of *scratching* would satisfy this criterion if it were the case that (a) if A scratches B and B scratches C then A scratches C and (b) if A scratches B then B does not scratch A. These two conditions would imply that if A scratches B and B scratches C then C does not scratch A. In actual fact this is very nearly true, and we can thus arrange substances in an order of hardness, saying that they are of the same hardness if neither scratches the other, and that if A scratches B then A is harder than B. However there are exceptional substances such that one of them A scratches a second one B, and B scratches a third one C, and yet C scratches A. In so far as these cases exist it is not strictly true that the relation of scratching determines a scale, that is, an order of hardness. Notice, therefore, that for an operation such as scratching to determine an order it is necessary for certain empirical facts about the world to be true. We cannot tell *a priori* whether an operation determines an order, though of course sometimes we may need only very familiar facts about the world to assure ourselves that it does.

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[2] *Physics, The Elements,* Part II, Cambridge University Press, 1920. *An Account of the Principles of Measurement and Calculation,* Longmans, 1928. See also the improved treatment by Hempel in 'Fundamentals of Concept Formation in Empirical Science', pp. 62-78, *International Encyclopedia of Unified Science,* Vol. II, No. 7, University of Chicago Press, 1952.

Neglecting the complication arising from these few anomalous substances that do not obey conditions (a) and (b) of the last paragraph, the relation of scratching enables us to place substances in an order of hardness. We could represent the hardness of substances by assigning numbers. Notice, however, that though we can assign numbers to represent degrees of hardness, it is arbitrary which numbers we use, so long as they are in the right order, that is, with the greater number to the greater hardness. For example, we could assign 1 to wood, 5 to glass, and 10 to steel, but we could just as well assign 3 to wood, 5 to glass, and 7 to steel. It thus makes no sense to say that steel is or is not twice as hard as glass, or even that the difference in hardness between steel and glass is or is not the same as the difference in hardness between glass and wood. We have got an order, or scale, of hardness, but we have not got a magnitude. That degrees of a property can be put in an order is a necessary but not a sufficient condition of the property's being a magnitude.

Masses, lengths, and periods of time can clearly be put in an order. For it is an empirical fact that if a mass A is put in one pan of a balance and a mass B is put in the other pan and A goes down, and if B is weighed against C and B goes down, then A will go down when weighed against C. Also that if A balances B then, if A goes up (or down) when weighed against C, B goes up (or down) when weighed against C. (What is a balance? Roughly, it is a thing for which certain empirical conditions are true, such that if A is put in the left hand pan and B is put in the right hand pan and they balance, then they will also balance if A is put in the right hand pan and B in the left hand pan. For the moment let us disregard the difficulty that these criteria for being a balance are always only approximately satisfied.) Similarly, things can be put in an order of length by means of the relation of overlapping. It is an empirical fact that most things are not like india-rubber: things like pieces of elastic apart, if A is put against B and overlaps it, and if B is put against C and overlaps it, then if A is put against C it will overlap it. Similarly, once more, with periods of time. 'The time taken to boil an egg (process A) is less than the time taken to roast a chicken (process B), which again is less than the time taken to eat a horse (process C)', can be taken to mean that if processes A, B, and C start at the same moment then A finishes before B and B before C. It is an empirical fact that processes can be found of which such conditions are invariably true. (Our example was a bad one in so far as there can be slow chicken-roasters and fast horse-eaters.) Provided that some such processes can be found, we can define an order in which periods of time can be put.

In order to get a *magnitude,* not just an *order,* we need some physical process corresponding to addition. Putting two things in the same pan of a balance is such an operation. For, denoting this operation by $+$, we have such facts as that if $A + B$ balances C, and $C + D$ balances E, then $(A + B) + D$ balances E. It would be tedious to go through the commutative, associative and distributive laws of addition in detail, but they all hold (approximately at any rate). Similarly, putting end to end is a physical operation that defines addition of lengths, and making one process start immediately another has stopped defines addition of periods of time. With hardness there is no operation with the required properties, and addition of hardnesses has no meaning.

Taking a mass S as a standard, any object that balances S is said to have a mass 1. Any object that balances two objects that each balance S is said to have a mass 2. An object that balances an object that balances two objects that balance S together with an object that balances S is said to have a mass 3. Two objects that balance one another and which together balance S are said to have each a mass $\frac{1}{2}$. And so on. The (rational) numbers which we assign to any given mass are not now arbitrary, or, more precisely, they are arbitrary only to the extent that it is arbitrary what object we choose for our standard—our unit mass. Any two systems of measurement of mass will differ only in that if M is the mass assigned in one system, kM will be assigned in the other system, where k is some constant.

Notice that fundamental measurement really is fundamental. In order to teach a savage what 'twice as long' means, you do not need first of all to teach him what it is to be twice as something or other else. (*A fortiori* you do not need to teach him any numerical laws of nature.) All you have to do is to play around with bits of wood, teach him what 'longer than' means, and teach him that an object A has the same length as an object B if they fit end to end, and that an object C has the length of A plus the length of B if B on the end of A gives us an object with the same length as C. Then, if he knows about fractions, you can select some piece of wood as a standard and teach him what a length of $5\frac{3}{4}$ is.

When we have our fundamental magnitudes we can define others in terms of them. For example we can define average velocity as the quotient of distance travelled by time taken to travel. That is, average velocity is defined in terms of two fundamental magnitudes. It is clear that, though fundamental measurement must always give us quantities expressible by positive rational numbers, negative, irrational, and even transcendental numbers may be required to express defined magnitudes. For example we could define y in terms of x as $\sqrt{x}$ or $\log x$. Sometimes the use of mathematics, which is analogous in some ways to a system of interlocking definitions, may make it necessary for us to use irrational and transcendental numbers even when ascertaining our original fundamental magnitudes. Suppose that a primitive people develops a system of fundamental measurement of length, and that, more intellectual even than the Greeks, these people develop systems of Euclidean and non-Euclidean geometry. They then make measurements of the lengths of the sides and diagonals of various squares and decide that Euclidean geometry is the one which is best applicable to reality. They then say that the length of the circumference of a circle of unit radius is 2π. So transcendental numbers are needed, though our primitive people have not defined any magnitudes not determinable by fundamental measurement. It is possible also by definition to define what are in reality quite new sorts of magnitudes. Suppose a primitive people has developed the fundamental measurement of mass and also a crude statics, in which they define the magnitude of a force in terms of the weight needed to balance it, and in which they define weight as proportional to mass. Unknown to them, however, this definition depends for its usefulness on a happy accident, the constancy, in the regions in which our tribe conducts its experiments, of the earth's gravitational field. The definition is not so stipulative as it looks. It is not *purely* stipulative, as is the definition of average velocity. This leads us to a type of magnitude whose possibility depends, quite openly, on the truth of certain numerical laws connecting other more fundamental magnitudes.

Such magnitudes are *derived magnitudes*. An example given by Campbell is *density*. The mass of a substance like water varies directly with the volume it fills. That is, $m \propto V$ or $m = \rho V$.

The number ρ varies according to the substance in question. The law $m=\rho V$ enables us therefore to define a new quantity, density, in such a way that a non-arbitrary number is assigned to a substance (non-arbitrary, that is, once the units of m and V are decided on). m and V are fundamental magnitudes, or alternatively V might not be itself a fundamental magnitude, but a fundamental defined magnitude (defined in terms of the fundamental magnitude *length*). This example of Campbell's may by some be found unconvincing. For it is difficult to know whether '$m=\rho V$' is a numerical law: it is suspiciously like an analytic proposition. For the procedure of adding masses (putting them together in a balance) whereby mass is defined is also a procedure of adding volumes. Nevertheless it might be that we could put masses of a certain substance together in such a way that they coalesced, and that equally we could separate them at will. If so we could define 'mass' in the usual way and '$m=\rho V$' would be false for this substance. Of course, this substance could not be used for our standards of length. m and V, it must be noticed, are fundamental magnitudes: they are defined purely phenomenologically. Later on we shall find m functioning as a theoretical magnitude. and in so far as it does so laws in which it enters may approach nearer to being analytic though they will never become quite analytic.

Once we have a number of derived magnitudes we can, of course, derive other magnitudes from them at a further remove. Let p be the pressure of a gas, v its volume. The relation $pv =$ constant enables us to use this constant as the measure of a new magnitude, absolute temperature. Again, suppose that we have already in some way defined current and potential difference. Then $\frac{E}{I} =$ constant (Ohm's law) enables us to define a new magnitude, electrical resistance.

We see that in order to introduce a derived magnitude we must already have established one or more numerical laws relating previously introduced magnitudes. These laws are experimental laws: so far we have not considered theoretical laws, and theoretical magnitudes.

For a derived magnitude there need be no physical operation corresponding to addition. It makes no sense to talk of adding temperatures or adding densities, and yet temperature and density can be introduced as perfectly good derived magnitudes.

There is a sort of magnitude that is somewhat intermediate between being a defined fundamental magnitude and a derived magnitude. Suppose that we are with our savage again, and that we have taught him the concept of length. We could then produce a uniform thin glass tube with a bulb on the end, fill the bulb with mercury, and mark the level of the mercury when the bulb is in melting ice '0' and the level of the mercury when the bulb is in boiling water '100', and then divide the intervening part of the tube into 100 equal divisions. This would give a meaning of a sort to 'a temperature of 70°' (indeed historically this is the form in which the notion of temperature was first introduced), but this procedure gives us a rather distant relation in the measurement family. How do we know, so far, that, if we made a thermometer using some liquid other than mercury, our two thermometers would give approximately the same readings in the same circumstances? The whole procedure is arbitrary. It does not depend on a previously established numerical law, as does the gas scale of temperature. Why should we not have made our gradations logarithmically, or perhaps, indeed, according to some quite odd mathematical law? (Perhaps a different law according to whether mercury or some other liquid is used.) That the procedure of defining temperature in this sort of way was fruitful is, from the point of view of logic, a happy accident.

The gas scale of temperature, so long as we only need it to be approximate, is a derived magnitude. (Theoretical considerations involving the concept of an ideal gas may, of course, be needed to refine it.) There is another scale of temperature, the thermodynamic one, which is of yet another sort. Temperature, so defined, is a theoretical magnitude. Another example of a theoretical magnitude is *force*. (In fact force is not a scalar magnitude but a vector, that is an ordered set of magnitudes, but this distinction need not concern us here.) Admittedly there might be a crude concept of force as a fundamental defined magnitude in an elementary and rough and ready investigation of statics, but force as it occurs in dynamics cannot be so defined. Dynamics as we know it today was first introduced to deal with the motions of the sun, moon, and planets. We determine the positions, velocities, and accelerations of the planets and in accordance with the theory of dynamics we deduce propositions of the form 'the force on such and such a planet is F'. The meaning of the word 'force' can be explained by explaining the functioning of the word in dynamics, but outside dynamics it is without a function. That is, statements about dynamical forces are meaningful only in the context of a theory. Similarly, gravitational, electric and magnetic field, wavelength of light, and nuclear charge are examples of theoretical magnitudes. If we open a book on physics we shall find that most of the magnitudes mentioned are theoretical magnitudes. Even more than derived magnitudes, they are sophisticated concepts. Before we could teach a savage what a certain derived magnitude was, we should have to get him to understand what certain fundamental magnitudes were, and we should have to demonstrate to him certain numerical laws relating these fundamental magnitudes. In order to teach a savage what a theoretical magnitude was, we should not only have to get him to understand, first of all, fundamental measurement and probably also a few numerical experimental laws and some examples of derived measurement, but we should also have to teach him a theoretical hypothesis, the statements of which, in conjunction with observation statements, would enable him to deduce further observation statements. The theoretical statements, however, cannot be asserted in isolation from one another, but only as part of a system, and statements assigning the values of theoretical magnitudes in special cases (say the gravitational force on the moon) would themselves be meaningless unless regarded as moves in the deductive game and not isolable from it. (Though I do not want to say that a theory is just a deductive game.)

We have made a few distinctions, and it is necessary to make them if we wish to be clear about scientific magnitudes. However, by a sort of feed-back process, which is very characteristic of science and which occurs over and over again in different ways, these distinctions get blurred as science proceeds. For example, mass is not entirely a fundamental magnitude, though it started, and had to start, as such. For accurate work we make corrections of our results of weighing by using theoretical considerations from mechanics and hydrostatics. And 'mass' in celestial mechanics is of course a theoretical concept. The picture can get blurred in other ways. Electric current was originally defined as a theoretical magnitude from which we can deduce the force on a nearby magnet (and force itself is a theoretical magnitude). Or, even more reconditely, electric current can be defined as rate of change of electric charge, and electric charge was originally an electrostatic concept. Maxwell's theory then enables us to deduce from propositions about the rate of change of electric charge certain observable consequences about the motions of magnets and of other wires containing currents. Having a definition of electric current, and setting up in some way a suitable definition also of potential difference, Ohm's Law enables us to introduce electrical resistance as a derived magnitude. (In this case we might call it a derived theoretical magnitude, as at least one of the magnitudes on which its definition *via* Ohm's Law is based is a theoretical magnitude.) On the other hand, electrical resistance could be introduced differently, namely as a fundamental magnitude. (There is a suitable physical operation corresponding to addition: putting in series.) Now suppose someone asks whether resistance is a fundamental magnitude or a derived one? There is no answer. From the point of view of theory it is more simple to define electrical resistance in terms of current and potential difference. From the practical point of view, however, resistance is much easier to measure directly than current or potential difference. So it is convenient to treat resistance sometimes as a fundamental magnitude and sometimes as a derived magnitude. A further feature which complicates the logical situation may be mentioned. If we measure resistance directly as a fundamental magnitude we do it by means of a Wheatstone bridge circuit. We arrange resistances in such a way that we get a null current through a galvanometer. Furthermore, we adjudge certain potential differences to be equal. Since to ascertain that a current is null need not be to make use of a system of *measuring* current, and since to adjudge certain potential differences to be equal can in this case be done on theoretical grounds without actually measuring these potential differences, resistance as measured by the Wheatstone bridge is not a derived magnitude (does not depend on the previous ascertaining of other magnitudes). But, though the Wheatstone bridge method could be taught to a savage as a system of fundamental measurement, as so taught it would seem to have an element of arbitrariness. This arbitrariness could only be removed by bringing in theoretical considerations. Hence, though there are good reasons for regarding resistance, determined by this method, as a fundamental magnitude, there are also reasons which would incline us to classify it a theoretical magnitude. An adequate taxonomy of measurement would be almost impossibly complex. Here we shall merely endeavour to indicate the sources of complexity and to understand them.

In order to see further how the distinction between theoretical and non-theoretical magnitudes gets blurred, let us revert to the concept of length, which is a good example of a magnitude which is sometimes a fundamental one, sometimes a theoretical one, and sometimes a bit of both. First of all length has to be defined as a fundamental magnitude (remember our savage on the desert island). We also define other fundamental magnitudes and discover numerical laws relating these, and furthermore we can define derived magnitudes and investigate further experimental laws relating these new magnitudes. We then invent abstract theories to explain these laws, and the magnitudes involved in the theory will be theoretical magnitudes. We can then use some theoretical standard as our standard of length, for example the wavelength of cadmium light. Nevertheless we must not forget that (a) it would be impossible to *begin* with this standard of length: measurements based on, say, the standard metre in Paris were needed before we could get the experimental evidence for our theory; and (b) we must have some concept of length which enables us to give meaning to the statement that the diffraction grating of the spectrometer has so many lines (evenly spaced) on it. That is, the wavelength of cadmium light may be used as a test for the correctness of our scales (even the scales on our spectrometer themselves), but without the scales in the first place we could never test the length of anything by our theoretical standard. The method is clearly one of successive approximation.[3] Our measurements of length may be modified further by means of the theory of errors. This is not a theory in the sense in which we ordinarily speak of a scientific theory: it is really a theory of pure mathematics (that is, it is a theory in the sense in which we speak of 'theory of functions', not in the sense in which we speak of 'quantum theory'). The mathematical theory of errors can be used to correct our measurements as follows. Sometimes the measurements we make, let us say by fundamental measurement, are 'subject to error': what this means is that beyond a certain degree of accuracy they do not give rise to simple experimental laws. The hypothesis of random or quasi-random error is introduced, and the readings are reduced to ideal measurements. What we measure in this way may be, near enough for our purposes, regarded as fundamental measurement. But it is not quite fundamental measurement. It could not be taught to our savage on the desert island without teaching him first the theory of errors (though this hardly counts as it is a branch of pure mathematics) and about empirical laws: he would have to learn the manner in which our original uncorrected measurements nearly, but not quite, fit these laws and how, if we correct these measurements in a certain way, they will fit them. So our resulting magnitudes are not prior to all experimental laws whatever, as true fundamental magnitudes are.

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[3] See Lenzen's: 'Procedures of Empirical Sciences', *International Encyclopedia of Unified Science*, Vol. I, No. 5.

The mention of the theory of errors naturally leads us to the question of what sort of things probabilities are. We can give probabilities precise numerical values, and when we say 'the probability of this badly made die falling with the 6 uppermost is 0·1357', the number 0·1357 is certainly a magnitude. It is, however, a theoretical magnitude. There are no empirical operations which give us the number 0·1357, in the way that weighing gives us the value of a mass: there is only a series of empirical tests whereby we can verify the *hypothesis* that the probability is 0·1357. Thus four sets of 10,000 throws, which gave the values 1357, 1358, 1357, 1356, would be regarded as verifying the hypothesis. If we got 1350, 1307, 1355, 1309 we should reject the hypothesis that the probability was 0·1357. (We might conceivably, in some circumstances, even reject the hypothesis that *any* probability could be assigned.) Hypotheses about probabilities are such that our rules of rejection for them must not be so final as for non-probabilistic hypotheses. (Though there are no *absolutely* final rules of rejection for those hypotheses which take the form of scientific theories.) We should not reject the hypothesis that the chance of 'heads' for a penny is $\frac{1}{2}$ just because when we try the penny we get five heads with five throws. This view, that statements about probabilities are hypotheses, is due to Braithwaite.[4] In effect Braithwaite recognises probability to be a theoretical, not an experimental, magnitude, and in this he seems to be right. You do not measure probability, but you do test the hypothesis that a certain numerical probability can be assigned to a property. This ties up with the fact that normally we do not talk of 'measuring' theoretical magnitudes. It is more natural to say that someone 'determined' a probability or the charge of an electron than that he measured it.

I have tried to distinguish various sorts of measurement and magnitude. We have fundamental magnitudes, defined magnitudes, derived magnitudes and theoretical magnitudes. We have also various hybrids between various of these. Corresponding to all these sorts of magnitudes we have various senses of 'measure'. And indeed, as was remarked in the last paragraph, the word 'measure' does not occur naturally at all in connection with theoretical magnitudes. There is one further sort of thing, however, which is commonly regarded as measurement, but which is such that, if it is regarded as a member of the family at all, it must be taken to be very distant relation indeed. This is the reading of calibrated instruments. A man who notices from the speedometer of his car that he is going at 25 m.p.h. is not making a measurement in any important sense of that word. The men who really measured were the men who calibrated the instrument. Instruments with pointer readings are not so much methods of measuring as technological devices to enable us to avoid measuring, when measuring is difficult. The simple question 'how are such instruments calibrated?' brings us back eventually to the question of fundamental measurement. When Eddington used to say that science was essentially concerned with pointer readings he never seemed to have asked himself the question of how his scales and dials were calibrated, and if he had asked himself this question he would surely not have believed his idealistic philosophy to be so consonant with the practice of science.[5]

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[4] *Scientific Explanation* (Ch. 5), Cambridge University Press, 1953.

Though I am here concerned with the philosophy of physics, the ideas of this article may have some useful applications to the philosophy of psychology. For psychologists frequently confess themselves to be worried about the nature of psychological measurement. They have frequently given in to theorists who have argued that psychological measurement is not really measurement at all, and that they were very naughty to have gone in for it, or else, more commonly, they have taken the attitude that, though psychological measurement is very naughty, nevertheless do it if they must. Few, however, seem to have had an easy conscience. As Sir F. C. Bartlett has said, 'When Theory whispers low "You can't", Practice replies "I must" '.[6] Where, then, do the sorts of measurement that we find in psychometrics come in? Are they of the sorts that we have described, or are they distant relatives of these, or are they perhaps illegitimate? My suggestion is that we should regard the magnitudes got from psychological testing procedures as theoretical magnitudes.

Consider I.Q. It is not a fundamental magnitude. For what physical or psychological operation corresponds to the addition of an I.Q. of 95 to one of 105? Furthermore, it is not a derived magnitude. For what are the fundamental magnitudes and what is the numerical law relating fundamental magnitudes from which it is derived? The suggestion may be made that I.Q. is not a magnitude at all, but only a scale (like hardness). This suggestion, however, is ruled out by the way psychologists treat I.Q. They treat it, and other quantities derived from test scores, as a magnitude, not just as an order, because they apply arithmetical operations, including addition, when they calculate correlation coefficients, moments about the mean, and so on. The only thing left seems to be to regard such magnitudes as theoretical magnitudes. That this view can be justified can be made plausible from a schematic example. Suppose a psychological test yields scores whose numerical values have a Gaussian distribution for the population. Suppose also that perfectly normal magnitudes like height and weight also yield a Gaussian distribution. We can then conjecture that the test scores correspond in a linear way to the value of some unknown magnitude. (Unknown at present.) Physiological research might enable us later on to identify it as, say, the number of neurons in a certain part of the cerebal cortex. Until, however, such an identification is made, we must regard the test scores as indicative not of an empirical, but of a theoretical magnitude. That we can play around with our scores as if they were indicative of values of a magnitude (e.g. by taking moments about the mean) depends on the acceptability of our hypothesis and this can be tested only by its fruitfulness.

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[5] See N. R. Campbell, 'The Errors of Sir Arthur Eddington', *Philosophy*, Vol. 6, 1931, pp. 180-192.

[6] 'Subjective Judgments', *Nature*, Vol. 166, pp. 984-5, 1950.

I suggest that some of the troubles that psychologists have got into over the nature of psychological measurement may have been due to their operationalism, which makes them look for a direct operational definition of a magnitude and to overlook the possibility that many apparently empirical magnitudes may, on a closer investigation, turn out to be theoretical ones. Furthermore, without a plausible theoretical justification at the back of it a method of psychological measurement is no more measurement than is racing to the rainbow's end a game. This is not because of any definition of 'measurement' or 'game', but simply because of the logical incoherence or incompleteness of the procedure suggested. The successes of psychometrics, however, are such that it is probable that in very many cases psychological magnitudes are perfectly respectable and that they are theoretical magnitudes.

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ANTHROPOLOGY AFTER FREUD[1]

By J. A. Barnes

Throughout his life Freud took a lively interest in anthropology. One of his last publications, *Moses and monotheism* (1939), deals with an anthropological topic. It is fitting, therefore, that we should look at the discipline of anthropology and try to assess how it has been affected by Freud's teaching.

I

Anthropology has changed greatly since Freud first became acquainted with the work of Frazer and others at the turn of the century. Among the many writers and thinkers who have helped to bring about this change, Freud must be placed in the first rank. His influence can be seen in the kinds of inquiries that are made in the field, the problems that are tackled, and the concepts utilized in analysis. Yet it is a significant fact that many of those works like *Totem and Taboo* (1912-13), in which Freud specifically sets out to examine anthropological themes, are seldom quoted by anthropologists and are comparatively neglected in anthropological teaching. Nevertheless it is mainly through these works that Freud's views on anthropological matters have been brought to the notice of his followers and the general public. To explain this paradox, we have to distinguish beween the influence of Freud on anthropology and the influence of anthropology, as mediated through the writings of Freud, on psycho-analysis. For, while anthropology and psycho-analysis have had considerable impact on each other, this impact has been, at least in the past, rather lop-sided. It is, I think, an index of the great profundity of Freud's general writings and of their thoroughgoing effect on anthropological interests and concepts that the special contributions made by Freud to the anthropological controversies of his day are no longer of central importance. The anthropological issues that were under debate during Freud's formative years have either been abandoned in favour of other interests or are now tackled quite differently. This shift of emphasis is in part due to Freud himself. Thus, for example, Freud was interested in the origins of totemism and set out his views on this subject at some length. I do not think that there is any anthropologist today who would subscribe to these views, and indeed we would do well to remember that Freud put them forward in a much more tentative way than he adopted when writing about events nearer home.[2] Yet totemism, without reference to its legendary origins, remains a topic of keen contemporary interest, and the mechanisms, such as projection, identification, internalization and the like, which anthropologists invoke in explanation of its various manifestations, are derived from Freud and his disciples.

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[1] Based on a paper read in Sydney on November 26th, 1956 at the Freud Centenary meeting of the Australasian Association of Psychiatrists, N.S.W. Branch.

At the beginning of the twentieth century anthropology was still deeply under the sway of evolutionary ideas derived from the work of Lyell and Darwin. Social evolution at its simplest became merely a search for origins. In England this line of development culminated in the work of Sir James Frazer, and it was by reading his *Totemism and Exogamy* (1910) that Freud's interest in the subject was sharpened. Today the quest for origins has been abandoned, at least in its old form, and anthropology is now more concerned with studying how orderly social relations are maintained between contemporary human beings loving and hating each other in an ambivalent way that would be quite out of place in Frazer's scheme of analysis. We are sometimes inclined to regret that, although Freud read Frazer's numerous works with great interest and insight, Frazer never felt it necessary to read a word of Freud. Yet perhaps in the long run anthropology has benefited from this failure to communicate. In effect it has meant that younger anthropologists have been able to turn to Freud not to study his contribution to ethnological controversies that have petered out, but to learn what he has to say about relationships between individual fathers and sons, mothers and daughters, husbands and wives. It is these findings which they have been able to utilize in their own work.

Freud's contribution to anthropological thinking has probably been greatest in the field of kinship, and we can best see in what way he has influenced modern social anthropology if we look at some of the relationships found within the family in various societies. Two factors determine this approach. On the one hand, Freud was profoundly impressed by the way in which a child's relationship to its parents forms the core of its personality development, and by the manner in which powerful emotions of love and hate strive for mastery behind the facade of domestic bliss and childish innocence. On the other hand, the study of kinship has long been one of the central interests of anthropology, and an interest which it has sustained largely alone among the social sciences. Hence a close link between Freud's teaching and kinship studies is to be expected.

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[2] Cf. *Totem and Taboo*, Standard ed., p. 102, footnote 1.

In the study of kinship, we are concerned with the way in which persons behave towards their parents, grandparents, siblings, spouses, cousins, affines and others. The pattern of relationships which prevails between persons related by descent or marriage varies widely from one society to another and is frequently markedly different from that found in our own culture. These variations have attracted the attention of many theorists, particularly since information about them began to become available in quantity at the beginning of the nineteenth century. However, the views advanced by nineteenth century writers were of necessity based mainly on speculation and fragmentary reports and not on detailed observation, and in consequence were of the nature of informed guesses, lacking verification. The same criticism can be levelled at all branches of nineteenth century anthropology, but whereas since the Renaissance and earlier there had been vigorous and informed discussion about such topics as the development of the State, the growth of economic activity and the nature of law, comparatively little had been written about the forms of the family. Even today, when contemporary political and economic systems are embarrassingly well documented, family life remains hidden in obscurity. Many of us, indeed, would argue that family life can flourish only in obscurity. Thus Maine, Bachofen, Morgán, McLennan and Tylor, the anthropological pioneers of the mid-nineteenth century, had few foundations on which to build their theories of kinship. Hence it is not surprising that kinship theories of the last century, with their preoccupation with group marriage and the ignorance of paternity, seem even further removed from modern anthropological analysis than is the case in the fields of primitive politics and economics. Yet these were the studies with which Freud was acquainted and which he tried to reinterpret in the light of clinical experience. Thus is comes about that, although Freud's lasting contribution to anthropology is certainly of greatest importance in the area of family relations in which he was most interested, Freud may not have expected the form it has taken.

II

Here there is space to mention only a few of the many topics that might be chosen to illustrate Freud's influence on anthropology and on the analysis of kinship systems. I shall try to demonstrate how his concept of ambivalence has been utilized in explaining certain customary behaviour between kinsmen, and how his ideas about symbols and about the significance of jokes have been adapted in sociological analysis.

A striking feature of many primitive societies is the requirement that persons who stand in a certain social relationship to one another shall avoid each other. Thus, for instance, a man may be enjoined never to be seen by his wife's mother, or a woman may be required to keep out of the way of her husband's father. A man may have to take care that he is never left alone with his sister. Sometimes these prohibitions are extended to include, for instance, not merely a man's real wife's mother, but also any woman who might properly become his wife's mother when he marries. The avoidance may apply to the possessions or attributes of the person avoided, as when a man is not allowed to sit on the sleeping mat used by his mother-in-law, or a woman has to walk round the outside of the cattle byre where her husband's father's cattle are kept. Occasionally the prohibition takes the form of a ban on performing the same role at the same time, as for instance when a woman is required to finish childbearing when her daughter starts to bear children.[3] Usages of this type have been reported from all over the world and are not associated with any one form of political organization. Rather, they seem to arise out of conflicts that are latent in every family and which have to be controlled in one way or another.

In most primitive societies a man and his wife are dependent on the coöperation of their kinsfolk and neighbours to a very much greater extent than they would be in our own society. Their social universe is comparatively small. There are few social services available to them other than the assistance provided by their relatives, and they cannot utilize the skills of the innumerable technicians and craftsmen on whom we rely. They call instead on their kinsfolk for help in securing their supply of food, in building homes, and in dealing with the crises of illness, birth, death, and the like. They may depend on their older relatives for land, livestock and other resources and for assistance in getting married. Coöperation of this kind is necessary, but it does not come about automatically. There is often resentment at the claims made by relatives on a person's time, energy and capital, and frequently there is a divergence of interest between the various groups of relatives who are expected to coöperate. Thus in many societies there is considerable tension between the social group in which a woman grows up and the group to which she goes as wife. Her own people may think that she is ill-treated and exploited by her husband's people, who on the other hand may suspect her of favouring her natal group and of deceiving her husband. In any event her loyalties are bound to be divided. One of the ways in which an open breach can be avoided is to reduce as much as possible the amount of contact between the two groups. Some contact is indeed often inevitable, but it may be possible so to arrange social life that certain key figures in the two groups never come face to face with each other. Thus, in societies where their direct contact is not essential, rules of avoidance may be found whereby a man and his mother-in-law are positively prevented from meeting. Many avoidance rules are concerned with the control of hostility between affines, but similar rules are utilized in other contexts. For instance, in many societies there is marked conflict between successive generations. In them we find rules of avoidance or rules to prevent members of adjacent generations from eating together, or openly competing with one another for positions of leadership.

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[3] Fortes, *Web of Kinship*, p. 223.

Rules of avoidance between affines, and between members of successive generations, seem to arise out of the social processes carried on within the domestic family. The family is the unit within which children are procreated and brought up in the culture of their parents and neighbours. In virtually all societies, the cultural values that are incorporated in the conscience or super-ego of the child include the belief that incestuous sexual relationships within the family are wrong and that a mate should be sought outside it.

In our own society we are generally content to limit the definition of incest to actual sexual intercourse, but many primitive peoples extend the prohibition to include a number of acts which are regarded as symbolically equivalent to sexual union. This extension provides a way in which the existence of the ban on sexual intercourse can be publicly, formally and persistently demonstrated, whereas we not only limit the ban, but usually endeavour to hide its existence. The possible reasons for this different approach are not immediately relevant here.[4] However, in many primitive societies, because of this widened concept of incest, a man or woman has to be careful not to perform actions which are regarded as symbolically equivalent to one or other variety of incest. A second kind of extension is found where it is considered incestuous for a person to have sexual intercourse with a close affinal relative, in particular with a spouse's parent or a spouse's child by an earlier marriage. In a monogamous system like our own, any abhorrence we may feel specifically towards sexual intercourse between people related in this way tends to be masked by our blanket disapproval of all extra-marital sexual activity; but even with us ecclesiastical rules about the remarriage of widows show that sexual intercourse with certain affinal relatives falls into a specially prohibited category. In those societies where polygyny is approved, and where some kinds of extra-marital sexual activity are permitted, the extension of incest to include intercourse with some close affines is of greater practical significance.

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[4] Cf. Glückman, *Custom and Conflict*, pp. 54-80.

In these societies the extension of incest beyond the nuclear family shows itself in two ways: prohibitions limit the choice of second wives, and prescriptions govern how a man (or woman) must behave towards his affines. The prohibitions on plural wives need not concern us here and I shall consider only the rules that lay down the proper way of behaving towards one's affines. Very commonly a man must avoid his wife's mother or must behave towards her with marked formality and deference. Similar rules sometimes apply to a woman and her husband's father. Behaviour that contravenes these rules is characteristically regarded as shameful or sinful, and as automatically entailing some terrible misfortune for the offenders and, sometimes, their relatives. Sometimes offenders are liable to punishment at the hands of some centralized authority or organized group of kinsfolk. But whether or not a penalty is imposed from outside, as it were, there is almost always a sanction imposed from within. The individual believes that to behave in this way would be wrong or bad, irrespective of whether or not he is found out, or alternatively he believes that, through some supernatural or mystical agency, retribution will automatically and inevitably follow the breach of custom. Breaches of custom occur, but they remain breaches, not customs, and the sanctions that uphold the customs are in the main internal sanctions, deriving from the conscience of the individual and the metaphysical beliefs that he holds, rather than external sanctions operating on the wrongdoer only when his crime is publicly known.

The internal sanctions that ensure conformity to the customary rules regulating the behaviour of a man towards his mother, sister and daughter in sexual matters arise, according to Freud, from the Œdipus complex and the derived abhorrence of incest. The evidence from primitive societies suggests that rules of affinal avoidance are sanctioned in a similar way. In a few instances the link with incest is recognized explicitly. For example, among the Nyakyusa of southwestern Tanganyika a married woman must avoid her husband's father. She must never look at him. If he approaches along a path, she must flee from it. She must not mention his name. Above all, she must not go near his bed, even in his absence. These prohibitions are extended to other affinal relatives. Nyakyusa explain their custom of making boys build new villages away from the dwellings of their parents by saying that it would be impossible for a man to live in the same village as his father; for if he did so, his wife and his father would not be able to avoid one another without an intolerable amount of interference with normal domestic living. To explain why a woman must avoid her father-in-law, they quote a myth in which a chief became enamoured of the wife of one of his sons. It is said to be 'very bad' and 'very rude' for a woman to see her father-in-law because 'it is as if you saw him sleeping with your husband's mother'. If a woman does go near her father-in-law's bed, Nyakyusa believe that she will bear a thin and sickly child.[5]

The same reliance on the sanctions of the super-ego is found with many of the regulations controlling social relationships between kin. Thus these rules are different in genesis and operation from those rules of avoidance where the main stress is on the differentiation of one group or category from another. For instance, in present-day South Africa many of the customs and laws governing social intercourse between members of different ethnic groups, and their utilization of public facilities, rely mainly on external sanctions. Naturally there are many instances where rules of avoidance not only are subject to the organized sanctions of political machinery, but also derive some of their force from beliefs about mystically-mediated disasters. Many of the rules concerned with the pollution of one caste by another in Hindu India seem to have been of this kind. Even if the penalties for breach of a rule of avoidance are almost entirely external, the rule may well be internalized in the super-egos of those who keep the rule. All systems of social control rely to a great extent on the bulk of the population feeling that it is wrong to do certain things, without stopping to calculate the penalty.[6] Indeed, when a breach of law ceases to be thought of as wrong, as well as illegal, we often say: 'The law has broken down'. We have come to look on many minor regulations in this light; parking regulations are an obvious example. All the fearful apparatus of the law, fines, imprisonment and execution, are demonstrably inadequate to maintain order if the consciences of the community are not on its side.

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[5] Wilson, *Good Company*, pp. 82-86 and *Rituals of Kinship*, p. 216.

[6] Cf. Nadel, 'Social Control and Self-Regulation', *Social Forces*, 31 (1953), p. 270.

The salient fact that emerges from an examination of the avoidance rules between kin is that within the family there is conflict. This conflict takes different forms in different societies, and is regulated in a variety of ways, but the common aim is to prevent conflict from continually flaring up and leading to a breach of social relations so that orderly life becomes impossible. Furthermore, as a result of the process of upbringing carried on within the family, the adult arrives at maturity with these culturally-approved mechanisms firmly embedded in his conscience or super-ego. It is in this way, rather than by the application of external force, that the norms of family life are upheld. This is hardly surprising when we remember that in all societies, but in many primitive societies to a much greater extent than in our own, the cultural tradition is transmitted to the growing child largely by its parents and other close relatives, and not by some impersonal agency like a radio programme, books or a government department.

Thus, whereas earlier anthropologists tended to stress the solidarity of the kin group against outsiders, later writers, following Freud, have paid greater attention to the stresses and cleavages within the group of kindred and in particular within the elementary family. In certain contexts kin groups do display solidarity, and indeed a great part of recent work in the field of kinship consists of the analysis of these contexts and of the social relationships they involve. However, while groups of kinsfolk, like other groups, may present a united front to outsiders, they are also segmented internally. The internal cleavages not only characterize relationships within the group, but also influence the behaviour of its members towards persons outside it.

III

Although Freud's influence is apparent in the analysis of rules of avoidance, I think his specific contribution is shown even more clearly in the modern treatment of what are termed 'joking relationships'. In many societies a man is enjoined to behave towards certain relatives neither with deference and respect nor with avoidance, but by playing practical jokes.[7]

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[7] Radcliffe-Brown, *Structure and Function*, p. 90.

We have a very attenuated form of this practice in our own culture. With us, a man does not avert his eyes if his wife's mother comes into the room, for there is no mother-in-law avoidance. But in popular phantasy, as revealed by songs and by the gags of professional comedians, we do make jokes about mothers-in-law, particularly the mothers of wives, even though few of us do in fact crack jokes at the expense of, and in the presence of, our own mother-in-law. However, in many primitive societies, a man does tease his grandfather or his mother's brother or someone else standing in some specified relationship to him. Typically, a man behaves thus to his wife's brothers. Thus a Cewa man sitting on the verandah of a hut with his fellows may suddenly turn to his wife's brother and say 'Brother-in-law, I see a louse in your hair'. As the brother-in-law starts to search for the louse the other bursts into laughter, as much as to say 'April fool'. Possessions belonging to a brother-in-law may often be removed with impunity, and he may be teased and abused in public. To take offence at such treatment is to indicate that one wishes to terminate the relationship. The joking may be one-sided, or it may be reciprocal.

Two of the points that emerge in connexion with these joking relationships are relevant here. The first is that relationships of this kind seem to arise in situations where there is a great deal of tension and latent hostility between the partners, but where there are other considerations which rule out the possibility of controlling the conflict by a rule of avoidance. Thus among the Cewa, mentioned above, it would be difficult for a man to avoid his brother-in-law. Often they live close together, they are likely to be of approximately the same age, and they have to coöperate in economic and social tasks in the community.[8] There are thus in their relationship elements of common interest as well as of rivalry and division. In this situation of ambivalence, joking provides a means whereby the hostility existing between the partners can find symbolic expression without disrupting the relationship. Instead, the relationship is reinforced. It is acted out in public in a manner calculated to attract general attention. A man may feel resentful because his wife is being influenced by her brother, and he then gets rid of some of this resentment publicly by making a fool of his brother-in-law. At the same time he demonstrates publicly that he is indeed his brother-in-law and that they both have an interest in the well-being of his wife and her children.

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[8] Marwick, 'The Social Context of Cewa Witch Beliefs', *Africa*, 22 (1952) pp. 128f.

The second interesting feature of many of these joking relationships is that in many societies they are specifically concerned with death. Indeed, some writers have described them not as joking relationships, but as 'funeral friendships'.[9] Another term used is 'cathartic alliance.'[10] In many societies the joking relatives are responsible for arranging certain details at a funeral. They may be called in to deal with the burial of those who are ritually unclean, like the victims of leprosy. In some societies they make jokes at the graveside, in a way reminiscent of the grave-digger in 'Hamlet'.[11] There is a wide range of situations in which joking relatives have tasks to perform, and it is difficult to generalize for them all, but on the whole it seems that one of the roles joking relatives commonly play is that of ensuring that social life is resumed after a serious realignment of relationships has become necessary, as for example after a death. They act, as it were, as an external coercive force in situations where the resources of the close group of kinsmen and neighbours are inadequate to cope with the situation. By their joking they provide a way in which the pent-up feelings of the kin of the deceased can find some relief. They do, of course, also provide social services in societies that lack specialized institutions such as undertakers, but that is not the point I wish to stress. What seems relevant is that in the presence of death, when the emotions of those closely connected are subjected to the greatest strain, and when a major adjustment has to be made in the social relationships of the group to which the deceased belonged, a number of persons come in from outside to provide support in getting the necessary tasks done, to relieve the mental tension, and to enable the bereaved to face every-day life again. The values of the society are reaffirmed and its continuity secured.

Joking relatives who appear at funerals are sometimes affinal kin, but they are sometimes recruited on some other basis, in terms of membership of a clan or age-set. A characteristic of joking relationships seems to be that they exist between persons, categories or groups that stand in some kind of ambivalent relationship to one another. Thus, as between relatives, joking relationships are maintained with those who are not so close that continuous coöperation is essential and yet not so distant that they can be avoided. Similarly, where there is a system of joking relationships between the tribes of a region, as in southern Tanganyika during the nineteenth century, joking relationships may be formed with those tribes who are not so close that permanent alliances are necessary and not so distant that permanent enmity is no disadvantage.[12] In all these contexts, joking enables hostility to be expressed symbolically. The ambivalence of affect is paralleled by the ambivalence of expression or action that constitutes the joke.[13]

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[9] Stefaniszyn, 'Funeral Friendships in Central Africa', *Africa,* 20 (1950), p. 290.
[10] Griaule, 'L'alliance Cathartique', *Africa,* 18 (1948), pp. 256ff.
[11] Colson, 'Clans and the Joking-Relationship', *Kroeber Anthropological Society Papers,* 8 & 9 (1953), pp. 53f.

IV

Another mechanism for the expression and release of social tension that has been scrutinized by anthropologists in recent years, and which is analogous to some of the clinical phenomena studied by Freud, is the reversal of roles in ceremonial. In primitive as well as in industrial societies, persons are organized for various purposes in hierarchical systems, with one person in authority over several subordinates and at the same time responsible to a superior of his own. There are often ceremonies forming part of the social life of these systems. One of the features which appears to distinguish many primitive societies from our own is that among them in the course of these ceremonies abuse is hurled at those in authority. Many actions normally prohibited are allowed, or even required, and in many instances people play roles strikingly different from those they follow in everyday life. Gluckman writes: ' . . . a most striking feature of their organization is the way in which they openly express social tensions: women have to assert licence and dominance as against their formal subordination to men, princes have to behave to the king as if they covet the throne, and subjects openly state their resentment of authority.' He goes on to say: 'Hence I shall call them rituals of rebellion.'[14]

Many of these ceremonial reversals of ordinary behaviour involve social relationships of superordination and subordination. It is widely realized that in any hierarchical system those in the middle of a chain of command are perennially in a situation of conflict. In one sense, whatever they do they are bound to displease somebody. A well known example is the foreman in a factory. There is a vast literature on the difficulties of his position, pointing out how he is supposed to represent the interests of management to the workers as well as the interests of the workers to management. If he always does what his work force wants him to do, management tend to regard him as obstructionist and uncooperative; yet if he does what management want him to do, his workers tend to regard him as a turncoat and a traitor who has been got at by management. Conflicts of this kind are in a sense inevitable, though they certainly need not be disastrous. They are one instance of a general pattern of conflict between rulers and ruled which is found in all societies. The short-term interests of the two groups are likely to be different. However, in most conservative societies the rulers and ruled have a common interest in the maintenance of the existing order of society. Even rival princes in dispute about who should be monarch all wish to uphold the monarchy. Indeed, they must uphold the monarchy, since this is the political system within which they struggle for power. Under these conditions we find that from time to time the conflicts in the society may be acted out in symbolic form. One of the clearest instances of this mechanism of social catharsis, as it has been called, is in the annual ceremonies of the Swazi kingdom, in which the king is, in pantomime, attacked by his subjects, accused of incest with his sister, and generally abused.[15] Similarly, when a new village headman is installed among the Ngoni, he must sit motionless with bowed head while his villagers and neighbours abuse him.[16] In both these instances, for a while the ruled become the rulers, and the ruler becomes the ruled. Yet this is done within a formally defined ceremonial setting which has a definite beginning and a definite end, and which is manifestly not part of every-day life. An opportunity is provided for the ruled to express some of their antagonism against the ruler; but also both ruler and ruled can express their conviction that the ruler shall be ruler and shall rule. Both elements in the ambivalent relationship can make themselves felt, and the relationship as such is confirmed and reinforced. There are obvious similarities here with the procedures of psycho-drama, but it is important to remember that, in these ceremonial situations, the intention is to reaffirm and consolidate an existing state of affairs, not to alter it. Perhaps it is this fact that partly explains why the formalized expression of hostility is absent from most of our own ceremonial life. Where the hostility of one group toward another is based on a real and long-term division of interest, where it is revolutionary rather than rebellious, then the symbolic acting-out conflict is unlikely either to affirm the existing relationship or to bring about the changes one or other group desires. It is perhaps significant that those few instances of ceremonial role-reversal which are found in our own culture occur in situations where the hierarchy of authority is not only emphasized, but is also generally accepted as right and proper. Thus it is in the army that we find officers waiting on the men for one day in the year, and in the navy the youngest boy in the ship dressing up as the captain, the next youngest as the commander, and so on in reverse sequence.

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[12] Radcliffe-Brown, op. cit., pp. 94f.

[13] Cf. Freud, *Psychopathology of Everyday Life* and 'Jokes and their Relation to the Unconscious'.

[14] Glückman, *Rituals of Rebellion*, p. 3.

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[15] Glückman, op. cit., and *Custom and Conflict,* pp. 122-136.

[16] Glückman *et al.,* 'The Village Headman', *Africa,* 19 (1949), pp. 101-4.

Thus, in the modern analysis of ceremonial, as in the treatment of avoidances and joking relationships, we see that emphasis is placed on the social relationships that prevail between actors in various situations, and on the conflicts, as well as the similarities of interest, that arise out of these relationships. We see how attention is given to the way in which the values and premisses of society are incorporated into the thinking of those who grow up within it, so that continuity and stability are preserved over the generations. The formalized actions of ceremonial and the spontaneous ambiguity of joking are seen alike as the expression of ambivalent affect in symbolic form. Clearly anthropology after Freud utilizes many concepts analogous to if not identical with those he developed; it did not employ them sixty years ago.

V

In any scholarly discipline new ideas flow in from a variety of sources, and it is often difficult to attribute a change of approach to any one innovator. This is particularly true of anthropology, occupying as it does a central position between the arts and the sciences and thus exposed to winds from every quarter. Freud was not himself an anthropologist, and therefore it is difficult to say exactly how much modern anthropology owes to him. Yet I hope that I have shown that in their analysis of social phenomena anthropologists owe much to the stimulus of Freud's teaching. In his treatment of dreams he drew attention to the importance of personally-defined symbolism, and thus opened the way for a re-examination of the rich ethnographic material on culturally-defined symbolism. He emphasized how the experiences of childhood colour the dispositions of maturity, and thus encouraged others to look at human beings as the end-products not only of millenia of cultural evolution, but also of their own personal experiences. Attention was shifted from conjectured historical processes stretching forward from the earliest human origins to short-term processes that could be seen or easily recalled. But Freud's greatest contribution to anthropological thinking arose from his insistence on the family as a field of conflict. Even in his own ethnological speculations, Freud postulated a family in conflict, the sons of the horde who killed their father and felt guilty for having done so. Writers on kinship before Freud treated the family in terms of arranged marriages and inherited property, and were not particularly interested in rivalry between father and son, between older and young siblings, or between father-in-law and son-in-law. Freud demonstrated how these phenomena could be studied, and interest in his approach was stimulated among anthropologists by Malinowski,[17] who emphasized the authoritarian role of the mother's brother among the matrilineal people of the Trobriand Islands. Further examination of the sources of intra-familial conflict has led not only to a deeper insight into these tensions, but also to a better appreciation of the mechanisms whereby, on the whole, families remain as units and do not burst apart.

At the beginning of this paper I drew a distinction between Freud's influence on anthropology and the influence of anthropology on psycho-analysis through Freud's papers on anthropological topics. When Freud published the first part of what was to become *Totem and taboo* he called his article 'Some points of agreement between the mental lives of savages and neurotics'. Anthropological field work during the last forty years has shown that savages and non-savages are much more alike than Freud may have supposed, but, together with the work of clinical analysts, it has also demonstrated that the phantasies and obsessions of neurotics throw into sharper focus tendencies present in all mankind, savage and non-savage alike. On the whole, I think that anthropology has gained more from psycho-analysis than psycho-analysis has gained from anthropology. Nineteenth-century anthropology has had a delayed impact on present-day psycho-analysis, but contemporary analysts have not, I think, taken the same lively interest in anthropological controversies as Freud took in his day. Far too little psycho-analytic work has been done among non-Western peoples and there are, particularly outside the United States, far too few instances of collaboration between members of the two disciplines. No doubt there are good reasons for this, but I believe that anthropology still has something to offer.

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[17] *Sex and Repression in Savage Society* (1927).

LAWS OF THOUGHT

By Leonard Goddard

When it is said that the laws of logic are necessary, it is implied that we must accept them. But why must we?

It is fashionable to answer this question in terms of linguistic conventions; one points to the meanings of the key words in them, words like 'and', 'not' and 'if . . . then'. A law such as the law of non-contradiction, 'not (p and not-p)', must be accepted simply because it follows from the definitions of the only two words in it, 'and' and 'not'.

Thus, whatever sentence 'p' we choose, if it is true then its negation, 'not-p', is false, and *vice versa;* and whatever two sentences 'p' and 'q' we choose, the compound sentence 'p and q' is true in case both components are true, false otherwise. These decisions (?) are recorded schematically in definitional truth tables,

p	not-p
T	F
F	T

p	and	q
T	T	T
F	F	T
T	F	F
F	F	F,

which, it is said, provide us with all the information we require in order to demonstrate the necessity of the law 'not (p and not-p)'. Thus, writing down all possible combinations of truth and falsity for 'p' (col. 1), we can then calculate the truth-value of the whole expression simply by reading off the appropriate values from the definitional tables. That is,

not	(p	and	not-p)
T	T	F	F
T	F	F	T
4	1	3	2

(the columns are numbered in the order in which they are calculated).

We see, therefore, that under all possible physical conditions, sentences of the form 'not (p and not-p)' must always be true (col. 4). For since 'p' may be replaced by any sentence, no matter what its meaning, and since all possibilities of truth and falsity are exhausted by the table (col. 1), both the meaning and the truth-value of any sentence which may be used to replace 'p' is irrelevant to the acceptability of the whole expression. Whatever sentence is used (whatever is the case in the world), 'not (p and not-p)' cannot fail. It is a truth which holds independently of the facts of life — as against, say, 'Stones fall to the ground when released from a height'; it holds solely by virtue of the definitions of 'not' and 'and'; it is a definitional, or verbal, truth. So, whereas, because the world might change, it is rational to doubt 'Stones fall to the ground . . . ', it would be irrational to doubt the law of non-contradiction. For *no matter what* changes take place in the world, it must always be true.

But suppose we press this a little and ask about these definitional truth tables. *Qua* definitions they are, after all, peculiar. One expects definitions to take the form, "the word 'not' means . . . "; but they are not like this. I might define the word 'bachelor' to mean 'unmarried man' and I should then be able to demonstrate from my definition that all bachelors are unmarried men. Here would be a definitional, verbal truth, in the ordinary sense. But is the law of non-contradiction like this?

No, it might be said, because the definition of 'not' as given by the truth table is implicit, not explicit like the definition of 'bachelor'. What is defined by the truth table is the *use* of 'not'. And with words like 'not', use exhausts meaning. An explicit definition, on the other hand, always in the last resort demands some non-linguistic knowledge before the meaning of the defined word is known. One might go on giving definitions of 'bachelor', 'unmarried', 'man', *etc.,* but in the end it would come down to marriage licences. Somewhere the dictionary has to be linked to the world, and the link must be known. Even if it is held that, with all words, meaning is use, still the meaning of a word like 'bachelor' is tied firmly to the variety of contexts in which it is (or may be) used; and linguistic contexts are not themselves linguistic, they are physical. The meaning of 'bachelor' is a resolution to use a certain sound or mark in a certain way, plus contextual references. The meaning of 'not', on the other hand, is just a resolution. Its use is context-free. And this simply strengthens the point that 'not (p and not-p)' is a verbal truth. What is meant by saying this is just that it follows immediately from implicit definitions which in themselves are neither more nor less than resolutions to use the words 'not' and 'and' in a given way; nothing more, no contextual reference, is required. Indeed the law of non-contradiction is itself an implicit definition of 'not' and 'and'. Taken along with all the other laws containing these words, it defines their interrelated use. Hence, there is a difference between the law of non-contradiction and the truth 'All bachelors are unmarried males', but it is a difference which operates in favour of the thesis that the law is entirely verbal in character.

But yet, one wants to reply, whose resolutions are these? And why *this* use (as given by the truth tables) rather than another? As a matter of fact, the word 'not' often is used in ways which simply do not square with the definitional table — go to any kindergarten! Is the definitional truth-table simply a summary of majority usage? If so, then, as linguistic habits change, the laws which follow from these definitions will also change. If definitional truth tables are generalizations based on the linguistic habits of particular sections of the community, then, in an important sense, logical laws are inductive laws descriptive of human behaviour. What happens in the world is relevant to their acceptability; for people may decide to talk in different ways. A last-calculated column of T's in a demonstrational truth table simply records that, as a matter of fact, the majority of people here and now use the words 'not', 'and' and so on, in such a way that, say, 'not (p and not-p)' is always true. Hence, though the behaviour of stones will not affect the law, the behaviour of people will; a change in linguistic habits will imply a change in the law. Where now is the 'must'?

But there is more to it than his. The laws of logic do not record (describe) human behaviour in the way in which the laws of psychology do; and they are not, as the laws of psychology are (or ought to be), held ready to be modified as human behaviour changes or as more information becomes available. For in logic we are dealing with resolutions, not with the way in which people do as a matter of fact talk but the way in which they have resolved to talk. Whose resolutions? The majority's, perhaps, though this is not important. Why this use rather than another? Because it is the *conventional* use. And this is where the 'must' comes in. For the resolutions, the definitions, are enforced. The child in the kindergarten who uses the word 'not' in an unusual way is told that he is wrong to do so; quite certainly his behaviour is not used as evidence against the law of non-contradiction. And there are sanctions in later life too. Social praise and blame, for example (as in the use of loaded words like 'fallacious', 'muddled', 'circular'; or, on the praise side, 'valid', 'clear-thinking', 'logical'); and in the last resort, the lunatic asylum. At the fallacious-muddled-circular end, the 'must' is the gamester's 'must': you must move the rook in a straight line. At the other end, the lunatic-asylum end, the 'must' is the policeman's "or-else" 'must': you must wear clothes in public. Play the (linguistic) game as the community plays it, or else! And of course there are a number of other 'musts' in between, from the mild socio-conventional (you must be nice to Mrs. Jones) to the stronger political (you must take your shoes off when you go into the temple).

But if the laws are conventional, they can be broken. Of course: people do argue fallaciously! Yet not only can they as the result of being ignorant, and making a mistake, but as the result of a deliberate decision. If I *choose* to talk differently, being entirely aware of the existing rules of the game, I should not be committing fallacies; I should simply be playing a different game. I might incur the wrath of the community, run the risk of not being understood, but that is all. My only problem, it seems, if I want to argue differently *yet logically,* is to persuade others to adopt with me a new set of conventions, a new set of logical laws. In this way I could set up my own linguistic community; and what is fallacious in the original community may not be fallacious in mine. For me, it may be, the law of non-contradiction would not be a law. It would fail *significantly.*

No doubt it would be difficult to do this, but, if laws are conventions, there is nothing impossible about it. It would be difficult; first, because that vast network of logical and grammatical laws and rules which serves as the *status quo* of discourse is so instilled — from the knuckle-rapping at school to the 'muddled'-sneers of later life — that it is taken for granted in all discourse; and secondly, because linguistic habits, being habits, are difficult to change. But not impossible; for conventions are alterable at will and by persuasion.

So we *can* talk (think, argue) differently, in the sense that it is possible. But we ought not to. This, finally, is the heart of the 'must'. And we ought not to for exactly those reasons that rebels always face. Upsetting the *status quo* always incurs social and moral reprobation.

This kind of view is not uncommon, and it has been taken up by C. A. Campbell in a recent article.[1] He takes as his text a quotation from Ayer:[2]

> "In the course of the debate Ayer agrees that one can't admit the possibility of a piece of paper's being white and not-white at the same time 'given existing conditions about the use of the word "not".' He goes on, 'But of course you could prefectly well introduce a convention by which it would be meaningful. Suppose you chose, when the paper was grey, to say it was white and not-white. Then you would have altered your logic. But given that you're using a logic in which you exclude "p and not-p", then of course you exclude the paper's being white and not-white'."

Campbell argues, it seems to me convincingly, against this view and in favour of the view that laws of logic are laws of thought in the old-fashioned sense (*i.e.* that they are necessary conditions of intelligible discourse). What he is concerned to show is that a convention such as

'This is white and not-white' is to mean 'This is grey' leaves the problem untouched since the *law of non-contradiction* has not been denied or changed; what has been changed is only the English language. To make good the view that laws of logic are conventional laws, it has to be shown that "it *is* possible to assert a self-contradiction intelligibly, if you alter your logic".

I want to add a few remarks by way of support on Campbell's side, and then suggest that nevertheless the laws of logic might yet be conventional in one sense (though perhaps an extended sense) of 'conventional'.

Logical laws are not *just* verbal conventions; at all events, not conventions like: the word 'different' should be followed by 'from'. They are necessary conditions of intelligible discourse in the sense that if I say "Stand up and do not stand up", my audience will experience a sense of frustration. They will not know what to do because they will not understand me, whereas they would understand me if I said, "This is different to that" — though they might frown. When I say, "Stand up and do not stand up", my words stifle action (except that the audience will laugh at me) unless, of course, I have already briefed them to stand on their heads when

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[1] "Contradiction: 'Law' or 'Convention'?", *Analysis*, 18.4, pp. 73-76.

[2] In *A Modern Introduction to Philosophy*, edited by Paul Edwards and Artur Pap, pp. 586-618.

I utter the 'command'. But in this case I should simply be using an unusual form of words to say, "Stand on your heads"; I should not be saying "Stand up and do not stand up"; I should not be uttering a significant yet contradictory order. Again, I often do say, "It is and it isn't", meaning by this, 'partly so and partly not so'; that is, I mean to assert that the alternatives put up to me are not exhaustive. Here again, however, I am not making a significant *contradictory* statement. On the contrary, I am saying that there is no contradiction involved in accepting both alternatives.

The dilemma is this: if logical laws are merely linguistic conventions, then a change (deliberate or otherwise) in our verbal habits ought automatically to entail a change in our logical habits. That is, if we change the meaning of 'not', argument forms which hitherto were valid ought no longer to be so, and those which were invalid ought now to be counted as valid. But this simply does not happen. If we adopted, say, Ayer's convention, then we should talk differently but argue and think as before, introducing a new word for 'not'. We are not changing our *logic* if the meaning of 'not' is changed in such a way that 'p and not-p' has a non-contradictory translation. On the other hand, if it does not have a non-contradictory translation, then we are not changing the meaning of 'not'. Logical laws are conventions, perhaps, but not verbal ones. Indeed, to show that they are conventions we have to show that we can on occasion assert 'p and not-p' while yet retaining that meaning (use) of 'not' which we ordinarily accept.

The point is the familiar point often made about arithmetic. Of course two plus three equals ten if 'two' means what we ordinarily mean by 'seven'. But can we have two plus three equalling ten if 'two', 'three' and 'ten' mean what they now mean? Can we count differently, not can we talk differently?[1] Analogously, the problem here is not, can we talk differently? but, can we think differently?

Yet if there is anything in this plea, it ought to be possible to point to some flaw in the argument which purports to establish the purely verbal character of the law of non-contradiction; and indeed there are defects. Thus consider again the demonstration of 'not (p and not-p)'. We are asked to accept:

(1) that the law follows immediately from the definitions of 'not' and 'and', and from these definitions alone;

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[1] Cf. Gasking, "Mathematics and the World", this *Journal,* Vol. XVIII, 1940, pp. 97-116.

(2) that these definitions adequately determine the use of 'not' and 'and' and therefore their meaning, since use exhausts meaning in such cases;

(3) that the acceptability of the law is therefore independent of the facts of life (excepting the linguistic facts of life), and hence that it is a definitional, or verbal, truth.

But both (1) and (2) are suspect.

Thus consider the first point, that the law of non-contradiction follows immediately from the definitional tables. It does, provided we first accept the law of non-contradiction. For if we are to accept the tables, and accept also the conclusion (3) which is drawn from the demonstration (that *no matter what* happens in the world, the law is always true), then we must accept that the rows of the table do indeed exhaust all physical possibilities. That is, we must accept that whatever sentence 'p' is chosen, there are only two possibilities for it: either it is true or it is false. But to say this is just to deny the possibility of any sentence being both true and false.

What is left out of the table is just as important as what is put in; and what is left out, provided it is agreed that it should be left out, amounts to an acceptance of both the law of non-contradiction and the law of excluded middle. For that we forbid rows of the sort,

p	not-p
TF	

which embrace the the possibility of p's being both true and false, and rows of the sort,

p	not-p
M	

(M = some middle value between T and F), which embrace the possibility of p's being neither true nor false, means exactly that the laws of non-contradiction and excluded middle are being taken for granted. These laws, that is to say, are essential presuppositions of the definitional tables, indeed of all the tables.

The demonstration, then, is circular. The law of non-contradiction does not follow from the definitions alone, it follows from these plus the laws of non-contradiction and excluded middle. Hence, even if the definitions were unexceptionable, the conclusion (3) would not follow. Before we can accept (3), we must accept the definitions and we must accept that the definitions imply the law. But the implication is suspect. So, too, are the definitions.

Thus, so far as (2) is concerned, the definitional tables adequately determine the use of the words 'and' and 'not' *provided* we already know how to use them. The schematic tables obscure this, but if we write the table for, say, 'and', in full, it immediately becomes apparent. For what we in fact have is,

if 'p' is true *and* 'q' is true then 'p and q' is true (1st row)
if 'p' is false *and* 'q' is true then 'p and q' is false (2nd row)
if 'p' is true *and* 'q' is false then 'p and q' is false (3rd row)
if 'p' is false *and* 'q' is false then 'p and q' is false (4th row)

Clearly, if we are to understand these hypotheticals, we must know what 'and' means; that is, we must know how to use it. And though one tried to gloss this by pointing to the fact that the italicised 'and' is strictly speaking a different word from that 'and' which occurs in the quoted expression 'p and q', since it occurs in a different level of language, still it has the *same use,* the conjunctive use. No-one, that is to say, could be taught the use of the word 'and' simply by presenting him with the definitional truth table. He would have to know this use before he could understand the table.

The table for 'and', then, is not a *definitional* table, because it is circular. And similarly, the table for 'not' is circular: For 'false' *means* 'not true'. That is, the rows are,

if 'p' is true then 'not-p' is *not* true
if 'p' is *not* true then 'not-p' is true.

The reasons for saying that the law of non-contradiction is solely a verbal truth, therefore, seem not to be good ones. Rather is it that the law is unavoidable in the sense that it seems to be presupposed in all discussions about it. And if this is so, then it is not simply a *verbal* convention; instead, it is a necessary condition of intelligible discourse, a law of thought. Much more than social pressure is involved if we consider changing it. For to deny it significantly is not just to introduce a new meaning for 'not' (or 'and'); to deny it is to show that, given the existing meaning of 'not', a contradiction is significant. I now want to suggest that perhaps this is possible.

I take it, first, that it would be generally accepted that the principle of contradiction *qua* law of thought, *i.e. qua* necessary condition of intelligible discourse, involves a spatio-temporal reference. For we can say, taking the ordinary meaning of 'not', that the same point of an object is white at one time yet not white at another, and we can say that different points of the same object are white and not-white at the same time. That is, strictly stated, the principle is:

Not both p and not-p at the same point and the same time, where 'p' stands for any statement which affirms a quality (*e.g.* 'X is white'). Or, if 'p' happens to be a statement which itself refers to place or time (*e.g.* 'X is here'), then the spatio-temporal rider is dropped or modified accordingly.

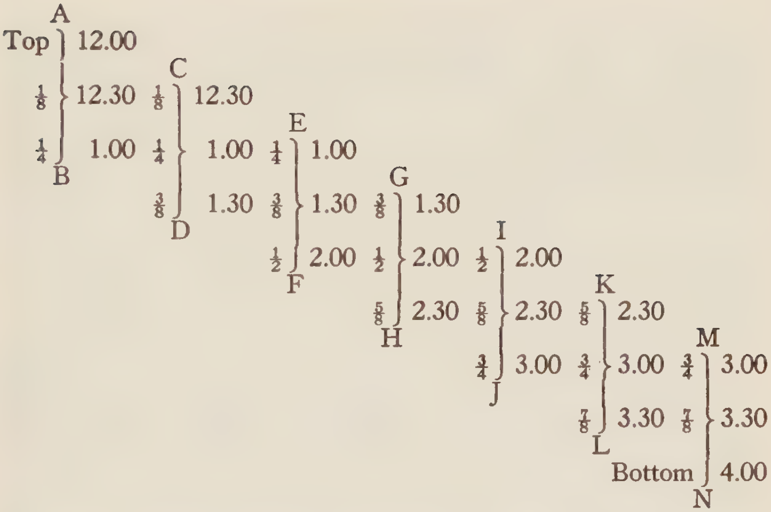
This being so, it may well be that if we change our conventions concerning space and time (whatever these are and supposing it possible for the moment) then the necessity of the law will be lost though we retain the ordinary meaning of 'not'. Given the ordinary meaning of 'not' and the usual space-time order, 'not both p and not-p' is a law of thought; we cannot think intelligibly except in terms of it. But, given the ordinary meaning of 'not' and a different space-time order, then, it may be, 'not both p and not-p' is not a law of thought; *i.e.* cases of 'p and not-p' are significant.

In order to illustrate this, I want to imagine that universe we all dream of, a universe in which it is possible to 'put back the clock'. Only, in the universe I am thinking of, not only is this possible, but things just happen that way. Natural processes repeat themselves according to a fixed pattern which is mirrored in clocks that work in the following way:

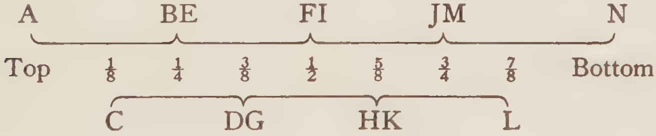
Starting at twelve o'clock, say, the minute and hour hands move in the usual way until they record one o'clock. They then continue from half-past twelve and move continuously through to half-past one. They then move from one o'clock to two; then from half-past one to half-past two; and so on.

This way of recording time is forced on the inhabitants by the ordinary pattern of events. A stone which rolls down a hill during the first occurrence of a given half-hour, will again roll down the hill in exactly the same manner during the second occurrence of that half-hour. Everything happens twice.

In general, therefore, we could simulate the measurement of time in this universe in terms of our own system of measuring time by using two continuous time-scales, one lagging a half-hour behind the other. Thus, suppose that the stone begins its journey down the hill at 12 noon, that it is $\frac{1}{8}$ of the way down at 12.30, and $\frac{1}{4}$ of the way down at 1 p.m. At the second occurrence of 12.30 it is $\frac{1}{8}$ of the way down, at 1 p.m. $\frac{1}{4}$ of the way down, and $\frac{3}{8}$ of the way down at 1.30. It continues in this regular way until it reaches the bottom. The pattern is as follows:



The two continuous scales are therefore made up in the following way:



In order to picture the techniques of such measurement, we could run two of our (continuous) clocks, one lagging a half-hour behind the other, each linked to a buzzer which sounds at the appropriate times (*e.g.* when clock 1 reads 1 p.m. and clock 2 reads 12.30) warning us to change from reading one clock to reading the other.

Within any one time-scale, therefore, since each is continuous and events happen consecutively, 'not (p and not-p)' will always hold; and even when both scales are taken into account still this will be true, since an event which occurs at, say, 12.45 the first time, is repeated exactly at 12.45 the second time. So no object is both white and not white at the same time either within one scale or in the extended sense of 'same time' which takes both scales into account. But the discontinuous moments are different.

For in this universe, space, or rather movement in space, as well as time, is discontinuous. The boulder which begins its journey down the hill at 12 noon and is $\frac{1}{8}$ of the way down at 12.30 and $\frac{1}{4}$ of the way down at 1 p.m., is back at the point $\frac{1}{8}$ of the way down at the second occurrence of 12.30. And this discontinuous change of position takes no time, otherwise neither space nor time would be discontinuous — the boulder would move so quickly as to defeat the eye perhaps, but still it would move through time, *i.e.* it would move *quickly,* and hence time could be measured on *one* continuous scale. Yet if no time elapses while the stone moves from B to C then at that moment it is in both places: *i.e.* it is at B and it is not at B.

At the moment before the discontinuous change the stone is approaching B, at the moment after the discontinuous change it is leaving C; at the moment of change, therefore, it is at both B and C. For from the continuity of the first scale it must reach B, and from the continuity of the second scale it must have been at C.

The principle of non-contradiction fails, therefore, in the form in which 'p' itself refers to position ('This is here and not here at the same time'). But it also fails in the form in which 'p' makes reference to a quality. If it happens to be raining at 1 p.m. but not at 12.30, then at the moment at which the stone is at both B and C each point of it is both wet and not wet.

Hence, although 'not both p and not-p' is a law of thought in the sense that it is a necessary condition of intelligible thinking about any moment other than the discontinuous ones, so also is 'p and not-p' a law of thought *qua* condition of intelligible thinking about the discontinuous moments, if movement happens to be taking place on both sides of them.

Campbell says, " . . . it would seem that, in logic, we are *obliged* to accept the rule 'p excludes not-p'; and if we are obliged to accept it, it is not a *convention.* It is a necessary rule for intelligible discourse — 'necessary', because we are just not able to think save in terms of it. But this is really equivalent to saying that it is a 'law of thought'."

I have wanted to show that we can accept the force of this in the sense of 'convention' in which Campbell intends it, but that the obligation on which he rightly insists, though it implies that the rule is a law, nevertheless does not exclude the *law* from being a convention in a wider sense. That is, 'not (p and not-p)' is both a law of thought and a convention. For since we can imagine a universe in which it fails, is seems clear that our ways of thinking, hence our laws of thought, are determined by the spatio-temporal conditions in which we live. Things happen in a certain way and this forces us to think in a certain way if we are to think intelligibly about those happenings. We are *obliged* to accept the rule 'p excludes not-p' because our space and our time are continuous, but, had the universe been different, so also might the obligation have been different, and so also might the laws of thought.

Yes, it will be said, our ways of thinking are determined by the spatio-temporal conditions in which we *live,* but not by spatio-temporal conditions which we *lay down.* Things just are so, and this causes us to think so. It has not been shown that, within our universe as it is, the laws of thought are conventional, because it has not been shown that the spatio-temporal conditions are conventional. If anything has been shown, it is, in an extended sense, Kant's point: that the necessity of our laws of logic and mathematics, hence the possibility of intelligible thinking, is firmly wedded to a space-time order. Only he wanted to say in addition that, for us, there could only be one such order. And this is the real issue. Is our space-time order a *conventional* order in the same sense that it is alterable in such a way that we still can control the happenings in the universe, that we still have a useful physics, geometry and logic? If so, then we can accept that, though we are obliged to think in a certain way if we are to think intelligibly, still the laws governing this intelligible thinking are conventional because the order in which they determine conditions of significance is itself conventional. But this has not been shown.

And in order to settle it, it would seem, it would have to be shown that we could look at the happenings in our world through different spectacles and "see" it as a discontinuous world; it would have to be shown that our ordinary way of perceiving is a conventional way, only one among many possible successful ways. The foregoing discussion is not evidence one way or the other so far as problems of this sort are concerned. For that a discontinuous universe is to some extent describable, and that we can understand that the inhabitants of it would have to think differently from us if they think intelligibly, is not by itself evidence that *we* could think and talk intelligibly within our universe in this fundamentally different way. Describing, recognising, understanding, thinking *about* the possibility of such a universe, is not the same as thinking *within* an actual universe of this sort. If we are to show that our space-time order is a conventional order, we must first change our ways of perceiving, hence our ways of thinking, hence our physics, geometry and logic.

Nor is the creation and use of non-Euclidean geometry a step towards this. Finite or infinite, linear or curvilinear, these are not the right kind of alternatives. Non-Euclidean space is not fundamentally different from Euclidean space in the way in which it would have to be to make good this point (or to constitute a refutation of Kant). Motions in it are still continuous, bodies are extended, and positions are distinct and uniquely describable. To make the point, or to answer Kant, a geometry which fails to satisfy conditions of this sort must be both consistent (in terms of the new logic) and usable.

And yet, suppose that there are, in the discontinuous universe, observers who are not themselves subject to the discontinuous changes but who "sleep", without being aware of it, in one-hour stretches, separated by one-hour stretches of wakefulness, in such a way that they miss all the discontinuities. Thus, these privileged observers are awake from 12 noon until 1 p.m. while the stone moves from A to B. Automatically, they then "fall asleep" for the period from 12.30 to 1.30, while the stone moves from C to D, and awaken at 1 p.m. (its third occurrence). They then stay awake until 2 p.m. while the stone moves from E (*i.e.* B) to F, when they automatically "fall asleep" again. Time and motion for these observers are continuous. For them, the stone rolls continuously down the hill from top to bottom since they only live (are "awake") within the first continuous time-scale from A to N. These people could not perceive differently, perhaps, but they could think differently if someone told them what the universe is really like. And they could alter their physics, geometry and logic accordingly.

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MODAL EXPRESSIONS IN ORDINARY AND TECHNICAL LANGUAGE

By R. HARRE

The study of modal logic ought to begin, though it often does not, with a preliminary study of the way the key expressions "necessary", "possible", "obliged", "must", "may" and so on are actually used. There is a tendency for logicians to rush into algebraic representation without any very clear idea what it is they are representing; with a consequent confusion of interpretations when the meanings of formulæ come to be considered. This unhappy state of affairs is usually, though not always, produced by there being too few distinctions made, too few use-families noticed. If some such preliminary study as I shall make here is used as a basis for representation and the creation of formulæ, purely linguistic difficulties are not so likely to upset the algebras later on. In such a discussion as this it is clearly impossible to cover the whole field of use of modal expressions and I shall confine myself to trying to make clear some central cases only.

It is not unreasonable to suppose that language was first used for doing rather than for saying, used to bring desired and desirable consequences about rather than to convey information. Language, it has been said, is fundamentally *performative*. Of performative uses of language surely the most fundamental is the command and, linked with it, the expression of obligation. Commands can be expressed by mood but the sense of compulsion they convey can also be carried by the modal auxiliary "must". "Go!" expresses a command in the imperative mood; "You must go" expresses, by the use of the modal auxiliary, both a command and the obligation upon you to go. (Consider the use of "You must go" as an *explanation* of the command.) The expression "must" can be used to turn an informative statement, "He goes" into the performative "He must go". It is important to notice that "He must go" is as much a performative as "You must go", for though it is not itself a command it may initiate the process that ends in a command by expressing a resolution. A board of directors decides to dismiss an employee in the words "He must go" and this decision is conveyed to the man in the command "Go!" The notion of obligation imported here by the use of "must" is perhaps the fundamental notion in the galaxy of concepts it is my purpose to elucidate.

The different modes or ways of making assertions is one of the features of language that the expressions of obligation have been used to mark. It is these that have mostly interested logicians and we must try to see what could be the point of using this particular notion of obligation in what seems a very remote context of use. Though the discussion of modes of statement-making goes back to Aristotle, we can quite profitably begin with the classification used by Kant.[1] He thought that there were three kinds or modes of assertion — the problematic, the assertoric and the apodeictic. The representation of these ways of asserting is one use of modal expressions. It is from this use that the current logical term "modal" comes. In English a representation of these modes is, for example:

Problematic	I	It may be red.
Assertoric	I	It is red.
Apodeictic	I	It must be red.

With the help of the word "true" we can express another similar gradation of assertion without interfering with the internal structure of the sentence modified. In the following examples the modal auxiliary is used not to modify the basic locution but to modify the ascription of truth.

Problematic	II	It may be true that it is red.
Assertoric	II	It is true that it is red.
Apodeictic	II	It must be true that it is red.

There is a strong temptation to assimilate these constructions one to another and to other constructions built in a similar way out of the apparently corresponding Romance expressions "necessary", "possible", etc. For example:

Problematic	III	It is possible that it is red.
Assertoric	III	It is the case that it is red.
Apodeictic	III	It is necessary that it is red.

and

Problematic	IV	Possibly it is red.
Assertoric	IV	Actually it is red.
Apodeictic	IV	Necessarily it is red.

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[1] *The Critique of Pure Reason*: abridged N. Kemp Smith; p. 74-75.

We shall have made a major advance in the study of modal logic if we can be brought to see that there are four different cases here.

That there is something unhappy about assimilating these sets to one another, tacitly carried through by the use of one kind of modal symbol for them all, is clear. Only through the problematic does there seem, even *prima facie,* to be a thread of genuine synonymity. Further, Apodeictic III is grammatically incorrect as it stands, for "it is necessary that . . . " takes the subjunctive in the dependent clause. There is evidently a case for more detailed investigation.

The basic modal notions of "command" and its complement "obligation" were expressed above by the use of "must", but they also find expression in the use of "necessary" and its cognates in various constructions. The analysis of these uses will be aimed at expressing what they convey in terms of obligation. I suggest that by analogy with persons being obliged to act in various ways we arrive at the notion of statements obliged to be true and events obliged to occur. The analogy goes by a steady gradation through obligations of various kinds on us to judge certain statements true, to bring about certain events, to a kind of logical and epistemological anthropomorphism by which we feel the statement to be obliged and the event to be constrained. We shall understand the use of "necessary" in these contexts when we have uncovered the source of the "obligation".

I suggest as a preliminary analysis of "Necessarily p" (in one sense) and of "p is (logically) necessary", "p must be true". This last can be paraphrased for the purposes of the investigation as "p is obliged to be true". What is the source of the obligation?

There are sentences which, whenever they are used, are used to make a true statement, and for which the truth of the statement made depends upon grammar and not upon context or content. A sentence *framed* in a certain way, we may say, *cannot but be* used to make a true statement; and statements made with this sentence *cannot but be* true. Here is a source of "obligation", for, given the grammatical structure, no alternative to a judgement of truth for statements employing this structure can be envisaged. The class of tautologies will be necessary in this sense; so that the precise sense "p is a tautology" can be given for "Necessarily p".

Another class of sentences which cannot be used to make false statements are those depending upon semantic generalities, that is those in which the meaning of the words and the grammar of the sentence combine to ensure the truth of any statement made with it. This group of sentences can also make some claim to give rise to statements "obliged" to be true, for, since their truth does not depend in any way upon contingent facts, they *cannot but be* true. This group of sentences and the statements made with them are those commonly called *analytic*. The class of analytic locutions will be necessary in the required sense; so that another precise sense, "p is analytic", can be given for "Necessarily p". It is usually held that the class of analytic statements includes the class of tautologies.

An interesting subsidiary case is that in which we call the conclusion of an argument "necessary" or say that the argument itself is necessary, or sometimes that the conclusion follows necessarily. Here we are marking the obligation upon us and everyone else to draw just *this* conclusion in a certain kind of argument.

The obligation to action finds expression in the use of "It is necessary that . . . " followed by a dependent clause in the subjunctive describing the action. There is also, with this construction, a strong feeling that the action described in the dependent clause is a condition for the attaining of some further end. "It is necessary that these buildings should be demolished" is quite unsatisfactory to an occupant as a *reason* for moving him out, since it is incomplete. He can ask "Necessary for what?" and legitimately expect an answer. The force of "necessary" here is still that of marking an "obligation", for it is used to indicate that, the ultimate end admitted or agreed upon, what follows in the dependent clause is an *indispensable* condition for the attaining of that end. The "obligation" is created by our acceptance of the aim or end prior to our grasping the indispensability of the condition, an indispensability which may be shown by the quoting of a law. If we want to attain the end we are, so to speak, *obliged* to accept the condition. A paraphrase into the simple "must" idiom works well enough in this case, for "These buildings must be demolished" can quite correctly be followed by "What for?", to which an answer in terms of some aim seems genuinely to be required. However, in certain circumstances the "must" construction could be a polite form of command, say in giving their instructions to a demolition team. "These buildings must be demolished" may be a polite form of "Demolish these buildings!"

"Possible" has a more complicated pattern of use than "necessary", for constructions in which it occurs can be synonymous with "may", "might" or "can" constructions, the uses of which are very different. The essential performative notion behind these uses is "permission". Permission is connected in a complex way with command and obligation. For an understanding of the notion of permission we need to introduce the idea of a negative command, a prohibition. A common expression of prohibition is to use the word "forbidden", as for example in "Smoking forbidden". However, an explicit prohibition expression need not be used. It is quite common to express prohibition with an emphatic negative — "No smoking", "No entry", etc.

It would be too simple an analysis to state an equivalence between "permitted" and "not forbidden", for actions which are quite indifferent are not forbidden either. There is a positive though weak sense of approval about most grantings of permission that is not conveyed by the simple negation of a prohibition. This sense of approval is sometimes quite strong but can be almost non-existent. However, whether this sense of approval is present or not, what is important for logic is that the granting of permission makes an action possible, in the sense that it becomes one of our alternatives, that is it becomes an action the doing of which without penalty we can now contemplate. From this central notion the concept of permission runs out into statements and events which are independent of actions, in much the same way as we have seen the concepts of command and obligation develop senses that through various analogies apply much more widely than in the context of human action.

If I am right in supposing the uses of "possible" and its cognates to be connected with the notion of permission, then we can look for the various uses of "possible" by a method parallel to that used in the discussion of "necessary". We can consider the two notions "permitted to occur" (the idea of permission applied anthropomorphically to events) and "permitted to be true" (the idea of permission applied anthropomorphically to statements).

"Possibly p", "It is possible that p" and "p is possible" are all ways of saying that p might be true, that p is "permitted" to be true. To say that p might be true is to say that now, at this moment, p is not certain. One way in which a statement may be less than certain is when it is neither tautologous nor analytic, and there is an important use of "possible", in the phrase "logically possible", that is often taken to have this sense. As we do not think that self-contradictory statements might be true the full analysis of "possible" in this sense must be "neither tautologous nor analytic nor self-contradictory". But the relation between "It is possible that . . . " and "It is true that . . . " is not quite so simple. In the discussion above it was assumed that "possible" is used not as an alternative to "true" but rather to modify a bald assertion of truth. It should be noticed that when "It is true that . . . " means "It is certain that . . . " and "It is possible that . . . " means "It is uncertain that . . . " then "It is possible that . . . " is exclusive of "It is true that . . . ". This is clear enough when we use the certain/uncertain idiom but it can be and has been confused when we do not use this idiom — for there is a temptation to assimilate all uses of "possible" to "logically possible", where "It is true that . . . " is not in conflict with "It is possible that . . . ". However, if we take "*p* is possible" to mean "*p* can be true", then in one sense of "*p* can be true" we are saying that *p* is the sort of statement which is verifiable, that is to say we are classifying '*p*' as a contingent statement or perhaps as a statement of a certain logical form, a form not such as to predetermine its falsity (as it would if *p* were contravalid in form). These uses are all cases where "possible" is taken to refer to the statement and not to what the statement is about.

The idea of events being permitted to occur, or not obliged not to occur, leads through a similar anthropomorphism to our calling an event or situation "possible" either because there *is* no reason why it shouldn't occur or because we know of no reason. In this use constructions with "possible" are equivalent to constructions with "can". "It is possible to climb Mt. Everest" is equivalent to "Mt. Everest can be climbed". Many people and most children are muddled about possibility in this sense and ordinary permission, and this shows itself in the general uncertainty about when to use "can" and when to use "may". This sense of "possible" can be paraphrased into "compatible with what we know" or sometimes "compatible with how things actually are".

It is important to see that "impossible" fits in with some of these uses of "possible" and not with others. For example, "impossible" does not seem to fit in with "possible" when the latter has the sense of "less than certain", for the opposite of "certain" is "uncertain", for which we use "possible". In this gradation there is no room for another opposite. With the other senses of "possible" above there are good uses for "impossible", for example in the sense "logically impossible". We can say a statement is logically impossible when we know that it cannot be true because it is self-contradictory in form. Similarly there is a use for "impossible" as the complement of "possible" in the sense (described in the last paragraph) in which we can say that a certain statement cannot be true, not because it is self-contradictory in form, but because it purports to describe something that is incompatible with the character of the world as we know it.

It is often thought (and indeed is the founding principle of some systems of modal logic[2]) that "necessary," "possible" and "impossible" have a sort of complementarity, so that where we can use, say, "possible" we should also be able to use "necessary" and "impossible" and say something systematically different, of course, but of the same general type. That this is true only of some special cases I shall try to demonstrate. A very commonly employed grammatical construction in which all three words can be used is "It is . . . for . . . to . . . ". It will become clear on the examination of some actual examples that while we may fill the first blank of the above schema with "possible" and "impossible" and say the same *sort* of thing, filling the gap with "necessary" leads to statements of an entirely different kind.

Roughly, statements made with sentences using this construction with either "possible" or "impossible" filling the first gap express the compatibility or incompatibility respectively of what follows with the known character of the world. On the other hand, the use of "necessary" with this construction expresses some kind of obligatory condition. To see the watershed here, consider the kind of question appropriate after "It is possible for Nilotic tribesmen to jump twenty-foot railings" and the kind appropriate after "It is necessary for Nilotic tribesmen to jump twenty-foot railings". The former seems to admit of questions of the form "How do they manage such a feat?"—"*How* is it possible?"; while the latter seems to require questions of the form "Necessary for what?"—"*Why* is it necessary?" "Possible" and "impossible" exhaust the field of compatibilities with the known character of the world; since something is either compatible or incompatible with the way things are, there is no third possibility. The use of "necessary" here puts us into another dimension of language altogether. Clearly the complementarity of "necessary", "possible" and "impossible" cannot unhesitatingly be assumed.

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[2] J. Lukasiewicz, *Proceedings of the XIth Congress of Philosophy*, Vol. XIV, p. 82.

A clue to the discovery of what cases are in fact complementary can be obtained by returning to the performative notions which I suggested lay at the back of the use of modal expressions. The three notions from which I have derived the constellation of uses so far set out in this chapter were "obligation", "permission" and "prohibition". There is a very clear complementarity to be found with these concepts and indeed they can be transformed into one another by the use of appropriate negatives. However, it does not follow that because a simple complementarity can be found in these underlying notions that it has been preserved through the hypothetical etymology I have set out above. Some of the uses to which "possible", "necessary" and "impossible" come to be put may be such that the complementarity is lost.

The following equivalences seem to be acceptable:

(1) *X* is obligatory is equivalent to *not-X* is not permitted.

(2) *X* is forbidden is equivalent to *X* is not permitted.

(3) *X* is permitted is equivalent to *not-X* is not obligatory.
There are other possible equivalences too. However, this is a simplification of ordinary usage, as are most schemes of codification, for in this table *X*'s which are obligatory are included in *X*'s which are permitted. In ordinary speech we are inclined to use the words "permitted" and "obligatory" as if we thought the permitted and the obligatory formed exclusive classes. This conflation could be argued for on the ground that if an *X* is obligatory it is certainly not not-permitted. That this is true in some cases in ordinary speech can be seen from the existence of the apologetic "I didn't say you had to do it, I just said you could". (The ambiguity between "This ought to be done" as a command and as a recommendation).

There seems to be only one case where "necessary", "possible" and "impossible" form a system of uses such as that described above for "obligatory", "permitted" and "forbidden". This is in the case where they are used with "logically" in "logically necessary", "logically possible" and "logically impossible". This is an artificial system of uses, but it can be given a kind of translation into those uses of "necessary" and "possible" where they modify the ascription of "true" and "false", and where "impossible" is used in the sense of "cannot be true"; that is, in the case where "cannot be true" is used to mark an impediment that lies in the form of the sentence used and not in the world. We might say that "logically possible", "logically necessary" and "logically impossible" form a special set of truth-values.

When "necessary" and "possible" are used to mark the force or status of a condition (and this is a highly artificial use with "possible") "impossible" certainly cannot be used. It makes no sense to classify a condition as "impossible". When we do say the conditions are impossible we mean that *taken in themselves* they purport to describe states of affairs incompatible with the known character of the world. (Or in mountaineering or blackmail that the conditions are more than we can or are prepared to consider.) In fact "possible" does not go with "necessary" here either, for we tend to contrast a necessary condition with simply "a" condition or perhaps "an alternative" condition.

The breakdown of any smooth transition between the various alternative expressions for the Kantian modalities I cited at the beginnng of this article can now be followed and explained in detail. We may dismiss Group I from this discussion, as they express modes of predication which we must take for granted, failing an analysis of atomic statements. Group II were those uses of modal expressions to modify the ascription of "true" and "false", which seem to be the only cases in which the complementarity of the underlying performative notions is preserved. I shall call this set the *logical* modes, for they are explicitly distinguished in technical discussions of logic (in contexts other than that of modal logic) by their being modified as "logically necessary", "logically possible" and "logically impossible".

In Group III "It is possible that p" and "It is necessary that p" are only superficially alike, for, in a real context where p is replaced by an actual sentence, "necessary" takes the subjunctive in the dependent clause and expresses the fact that the dependent clause is a condition for the attainment of some further end or aim. The similar construction with "possible" has no such sense but simply marks the compatibility of p with the nature of the world. This use of "possible" I shall call the expression of *empirical* possibility, in contrast to the use above, which is logical possibility. Similarly I shall contrast the necessity of an antecedent condition with the necessity to be true as the contrast between *anteconditional necessity* and *logical* necessity. (There is no sense to be given to empirical necessity except in the special way I shall discuss below.)

The adverbial uses of Group IV seem to express just what was expressed with auxiliaries in Group I. For example "Necessarily it is red" seems to express what is said by "It must be red" and "Possibly it is red" by one of "It may be red", "It might be red" or "It can (could) be red". It would be a mistake to suppose that there was a genuine complementarity in Groups I and IV. "It must be red" is used to give an order, to make a plea etc., while "It might be red" is used give expression to an envisaged possibility, or to express uncertainty about the colour of something. "It may be red" can be used to give permission for the colouring of something, and in this use *is* complementary to "It must be red". However, it is much more commonly used to make a guarded statement, similar to but not quite the same as the use of "might". Finally, "It can (could) be red" is commonly used as a direct statement of an alternative, expressing the speaker's belief that there are no obstacles in the way of the thing's being or becoming that colour.

The word "may" seems to deserve a closer analysis. This word can be used to do two different jobs. It has an independent use as a performative signal in the asking for and granting of permission. "May I go?" — "Yes, you may go". However, it is also used as an alternative to "will" in such statements as "It may rain" or "There may be war". It is used here, I think, as a way not of making a tentative prediction (the function of "might") but as a way of tentatively making a prediction. It is used to express hesitancy as opposed to probability or uncertainty (uses of "might").

The various ways in which modal concepts are used outside their original deontological setting can be summarized in a table. "O", "P" and "F" stand for "Obligatory", "Permitted" and "Forbidden" respectively, and I use them in the tables to indicate the *hypothetical* ancestry of the concepts there classified. An expression in brackets is an alternative that strikes me as less common.

Logical Modalities.

	L I	L II
O	necessary (valid, analytic etc.)	must be true
P	contingent (possible, synthetic etc.)	can be true
F	self-contradictory (impossible etc.)	cannot be true

	L III	L IV
O	necessarily true	logically necessary
P	possibly true	logically possible
F	necessarily false	logically impossible

Empirical Modalities.

	E I	E II
O	—	—
P	possible	could (can) be true
F	impossible	couldn't (can't) be true

Epistemic Modalities.[3]

	EP I	EP II	EP III
	certain (definite)	is true	true
O	—	—	—
P	possible (uncertain)	might be true (may be)	possible
F	—	—	—

Anteconditional Modalities.

	A I	A II
O	necessary	necessary
P	is an alternative (sufficient)	unnecessary
F	—	—

Tentative Assertion Modalities.

	TA I
	will
O	—
P	may
F	—

In "true", "false" etc. we have a set of expressions the purpose of which is to express or mark the result of making an appraisal of the success or failure of statements and other locutions in fulfilling the task they are used for. The above expressions are used in constructions like "It is true that . . . " and "It is false that . . . ", which mark the result of making the appropriate appraisals of statements. It seems clear from the analysis so far that the modal notions of obligation, etc., are sometimes put to work in expressions like "It is true that . . . " and "It is false that . . . " to mark the results of a special class of statement-appraisals. It will now be obvious that this use does not cover all the ways in which modal notions enter our language, but it is an important way. In formalizing the use of modal expressions they must, therefore, in some cases be assimilated to the type of the truth-values of logic. From this preliminary conclusion about the logical status of modal expressions further conclusions can be drawn by a fuller discussion of the scheme set out above.

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[3] I borrow this term from G. H. Von Wright: *An Essay in Modal Logic*, pp. 1 and 2.

Logical Modalities. The sets of expressions in these use-families can be used for a variety of purposes.

1. They can be used to mark the outcome of the appraisal of a statement. This is clearly the case in such examples as "What he said must be true"; "That story can't be true".

2. It is usually the case that the appraisals marked by the use of these expressions are made, not on the basis of the "fit" of the statement and the facts, but on the basis of the internal structure and constitution of the statement; even, in some clear-cut cases, on the basis of the structure of the sentence used to make the statement. For example, a statement must be true if it is tautologous or analytic, and so tautology and analyticity in statements are *grounds* for the judgement expressed in "*p* must be true". Similarly a story can't be true if it contains some internal self-contradiction, if the teller of the tale contradicts himself, and so self-contradiction is a ground for the judgement "*s* can't be true". Clearly "must/can/can't be true" are not predicates which can be used to state the grounds upon which the appraisals they express are made; nor are the parallel forms "necessary", "contingent" and "self-contradictory" *when they are used as synonyms* for "must/can/can't be true".

The expressions of L IV, e.g. "*X* is logically necessary", are ambiguous unless carefully specified, for wo do not know *prima facie* whether they express a judgement arising from an appraisal of a statement or whether they are descriptive of the logical character and structure of *X* and hence serve as the grounds for such judgements. Similarly when we say "*p* must be true" we may either be appraising *p* or describing '*p*'. There is a still deeper complication inherent in these uses, for I have been talking here of statements, but the character of a statement with respect to necessity, contingency etc. is dependent in many cases upon the structure and grammatico-syntactical form of the sentence used to make the statement. Sometimes the expressions classified under logical modalities are used to describe the character of sentences, the sentences used to make the statements which "must/can/cannot be true".

Empirical Modalities. A similar ambiguity between judgement and grounds of judgement exists with the use-families E I and E II. While "can be true/can't be true" can only be used to express the judgements we make of statements (and metaphorically of sentences), "possible/impossible" can be used to make statements forming the grounds of these judgements. In this case the grounds lie not in the form of the statements made but in the compatibility of what is described in the statement with the known character of the world. I discussed above the case of certain tribesmen jumping railings. If it were said that they could jump seven-foot railings we might comment upon what is said with "Yes, it's just (quite) possible", for the situation imagined is compatible with the known character of the world, with what we know of athletic performances. However we would unhesitatingly say of the statement that they can jump *twenty*-foot railings that it can't be true. And the grounds for this judgement of the statement is the incompatibility of what is claimed with the facts of human athletic performance. It is worth noticing here that the impossibility of the alleged performance is not so strong as when a self-contradiction is the grounds for the adverse judgement, for we do not know *all* the facts about the world.

Epistemic Modalities. The pairs "certain/uncertain"; "true/possible" and "true/might be true" belong in the same dimension of language as "true" and "false", though they are used to express different dichotomous judgements. "True" in EP II and EP III seems to be used as shorthand for "known to be true", while "certain" and "uncertain" have a similar use. It is the expressions "known to be" and "might be" that are modal, for they qualify in certain ways the ascription of truth, that is they are adverbial in function. There is an analogy between "known to be" and "necessarily" that is not shared by "certain"; for with "know" we import the notion of obligation which is one of the basic modal notions. That which is known to be true is "obliged" to be correct; otherwise a serious impropriety has been committed by him who says he knows. "I know that . . . " is a performative formula that deserves an independent study.[4]

Anteconditional Modalities. These expressions are ways of bringing out the status of conditions. "Necessary", marking a condition without which the consequent is not to be achieved, is a natural extension of the basic deontological notions, for a necessary condition is an indispensable or "obligatory" one. A condition which is one among alternatives must, if it is to be a genuine alternative, be a sufficient condition. We do not usually use a modal adverb to mark this kind of condition, and it seems that this linguistic fact is connected with the meaning we commonly attach to "condition". A condition that is not a *genuine* alternative, say for a lack of sufficiency or perhaps irrelevance, we would hardly in ordinary language want to call a condition at all. The use of "unnecessary" in A II is connected with this point, for we use this word when we want to say that something or some action *serves no useful purpose.*

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[4] J. L. Austin, "Other Minds": *Logic and Language, Second Series.* Ed. A. G. N. Flew, (Ch. 8). This contains an admirable account of the use of "know".

Tentative Assertion Modalities. The use of "may" to express a tentative assertion is a linguistic device somewhat similar in function to the use of "not" to express a negative assertion. That this is different from the use of "might" can be seen in the fact that "might" belongs in that spectrum of probability which begins with "certain" and tails off into uncertainty.

I suggested by the bracketing of "can" in E II that "can" is not so commonly used as "could". This is connected with the fact that we usually use "possible" and "impossible" in the envisaging of situations rather than in their description. There is thus very often a predictive element about the use of "possible" and "impossible", and this it is that tends to make us use "could" rather than "can" as the correct equivalent. "I can see now that it's possible", said as we watch the tribesman breasting the twenty-foot railings, is equivalent to "So it can be done", whereas an academic discussion of athletic possibilities must be expressed in terms of "could". There are fine shades of meaning here, for there are cases in which we express our wonderment when we had been *so* sure, as in the statement "So it could be done after all". This expression serves to refer back to our previous conviction that it couldn't be done, and I think simply reinforces the general analysis.

Natural Necessity. I suggested in the early part of this discussion that, though a good sense could be given to empirical possibility, natural or empirical necessity could only be given a sense in a very special case. This is important for modal logic, for if we interpret "possible" as an empirical modality the apparent complementarity between "possible" and "necessary" may lead us to interpret "necessary" in an empirical way. "Natural necessity" does not play any serious part now in the philosophy of science, but it does seem to enjoy a sort of half-life in uncritically interpreted modal logics because of the apparent complementarity above. (For example, it looks very much as if A. N. Prior is thinking of some sort of natural necessity in *Formal Logic,* p. 187, when he constructs a table of *general* equipollences between the various modal concepts.) There *is* a use of "must", and more rarely of "necessary", in scientific statements which deserves investigation. At an uncritical glance the use of these words might suggest that scientists were saying that certain phenomena *must* be the way they are; that they are "obliged" (by some natural necessity, as we might say) to occur the way they do. The word "must" occurs in this sort of example: "Given that the initial velocity of a bus is 10 feet per second and that it accelerates for 5 seconds at 2 feet per second it must have a velocity at the end of 5 seconds of 20 feet per second." (It is worth noticing that we do not commonly stress this "must" when *talking* like this.) I think it is proper to regard the necessity expressed in this use of "must" as coming from a rule or method of inference by which we infer the final velocity of the bus. If we accept a certain law of nature, in this case '$v=u+at$', as the proper rule for making predictive inferences about the kinematics of buses then *within the context laid down by this law* the conclusion follows necessarily. We are obliged to draw just this conclusion on pain of misusing the laws of nature and the rules of arithmetic. Clearly this does not further oblige us to accept "natural necessity", for that would mean accepting '$v=u+at$' and other laws of nature as themselves necessary. To this extent logic and physics are not parallel, for the general rules of inference which are the theorems of axiomatic logic are themselves logically necessary.

A distinction in kinds of modality was drawn by medieval logicians in terms of the modes of predication *de dicto* and *de re;* concerning a speech and concerning a thing. Clearly necessity can *only* be predicated *de dicto,* whereas we can say that possibility can be predicated both *de dicto* and *de re,* for "possible" can mean either "logically possible" or "empirically possible". This is still a gross over-simplification, as I have tried to bring out in the analyses above, but it serves to emphasize the point that only in the logical modalities is there a genuine complementarity between "necessary" and "possible". The attempts by modal logicians to express a *general* complementarity between modal notions using the language of "necessity" and "possibility" can only succeed at the price of an almost total distortion of the language they are supposed to represent.

Conclusions.

1. "Necessary", "possible" and "impossible" can only be used in the same scheme of interpretation for a logic when they mean: *either* "logically necessary", "logically possible" and "logically impossible", and here they belong in a *meta-language* used to characterize the statements of which they are predicated; *or* "must be true", "can be true" and "can't be true", and here they are of a type with the ordinary ascriptions of truth and falsity and must come into logic as special *truth-values*.

2. There are many other uses of these words in our language, but in none do we find any kind of complementarity between "necessary" and "possible"; and it is on this complementarity that most systems of modal logic are based. It seems then that it is not possible to represent modal notions in a propositional calculus without giving that calculus an interpretation as a meta-language for a propositional calculus of a lower level.

3. A useful line of approach may lie in the concepts of "obligation", "permission" and "prohibition", but this raises new problems of analysis which are outside the scope of this article.

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DISCUSSION

DO WE EVER VALIDATE MORAL STATEMENTS?

I

By Robert Hoffman

Mr. D. H. Monro, in a critical notice of Dr. P. Edwards' *The Logic of Moral Discourse,* suggests a position which, though mistaken, is illuminating.[1] He maintains that if fundamental moral judgments[2] are logically implied by every moral judgment, and if we cannot validate them, we cannot be said ever to validate conclusions of moral arguments.

Now it is true, of course, that the validation of any non-fundamental moral judgment terminates with the exhibition of the principles that govern the realm of argument concerned. And, by definition, the principles which serve as fundamental moral judgments, i.e., the ultimate validating principles, cannot themselves be validated. Yet it does not seem to me to follow that we therefore cannot validate our non-fundamental judgments.

I think that Mr. Monro fails to take into account the practicality of the moral situation. What is called for is a decision here and now in terms of our doubts in particular circumstances: the doubts removed, we decide. And it must be emphasized that we do not begin with complete doubt; we bring to a situation certain funded values which we do not doubt. And, as Peirce points out, what we do not doubt, we regard as true. Moreover, doubt, when it is voiced, must be specific; it cannot be a general Cartesian doubt. This aspect of the matter has been put clearly by Dr. L. G. Miller:

"A request for justification [i.e., validation of a moral statement or act] is legitimate . . . only if the critic has grounds for doubt. When he questions the position it is implicit in his questioning that he has reasons for doubting and that it is in principle possible to remove this doubt by showing that it is unfounded. If this is not the case there would be no point in asking for justification. Furthermore, the critic cannot expect us to answer him until we know what reasons he has for doubting. Continued doubt is justified only if he has some grounds for doubt. If a person asks us to justify a moral statement it is always proper to ask him what his objection is and on what he bases it. If someone argues, 'No matter how many specific objections you answer, the validity of this judgment will still be in doubt' or 'Quite apart from specific objections, this judgment can still be doubted', he shows an unawareness of one of the basic characteristics of moral reasoning. Justification is associated with specific criticisms, whether actual or possible. If the critic is unable to raise specific objections against a judgment, or unable to give reasons why he thinks there may be objections as yet unrevealed, he cannot reasonably doubt the validity of the judgment. The judgment is not just probably valid, or highly valid — it is valid."[8]

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[1] This *Journal,* Vol. 34, No. 1, May, 1956.

[2] Dr. Edwards defines a fundamental moral judgment as follows: "When a man is unable or unwilling to support a moral judgment with anything that would be considered a reason we shall say he has made a fundamental moral judgment. When and in so far as he can support a moral judgment with a reason, we shall refer to it as non-fundamental" (pp. 182-183).

If, to the question "What have you in mind as an alternative position?", the critic of our judgment can offer no reply in terms of a judgment and reasons therefor, he is not (logically) entitled to entertain doubt. The presumption of truth for our judgment stands until he overrules it by a countervailing claim, i.e., an opposed judgment supported by a reason or reasons.

But suppose countervailing claims are urged until the moral dispute in question is fundamental. Moral conflicts of this sort *are* incapable of being settled in any rational way. Doesn't this show that moral disputes are, after all, irrational, for any dispute can be imagined to be pursued until it becomes a fundamental one? I think not.

First of all, validation must come to an end somewhere or else it would not be validation at all. Consider the problem with which the Tortoise confronts Achilles in Lewis Caroll's "What the Tortoise said to Achilles". He offers Achilles three propositions, A, B, and Z:

(A) Things that are equal to the same are equal to each other.

(B) The two sides of this Triangle are things that are equal to the same.

(Z) The two sides of this Triangle are equal to each other.

Then he tells Achilles that he wishes him to suppose that he (the Tortoise) accepts propositions A and B as true but does not accept the hypothetical proposition that, if A and B are true,

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[8] Miller, L. G., *Some Problems Concerning the Justification of Moral Rules.* Unpublished Ph.D. Thesis, Cornell University, 1954, p. 155; cf. Toulmin, S., 'Knowledge of Right and Wrong', *Proceedings of the Aristotelian Society,* Vol. L (1949-1950), p. 155.

Z must be true, and then force him logically to accept Z as true. The following dialogue ensues:

A: I'm to force you to accept Z, am I? And your present position is that you accept A and B, but you DON'T accept the Hypothetical . . .

T: Let's call it C.

A: . . . but you don't accept: (C) If A and B are true, Z must be true.

T: That is my present position.

A: Then I must ask you to accept C.

T: I'll do so as soon as you've entered it in that notebook of yours. Now write as I dictate: (A) Things that are equal to the same are equal to each other. (B) The two sides of this Triangle are things that are equal to the same. (C) If A and B are true, Z must be true. (Z) The two sides of this Triangle are equal to each other.

A: You should call it D, not Z. It comes NEXT to the other three. If you accept A and B and C, you MUST accept Z.

T: And why MUST I?

A: Because it follows LOGICALLY from them. If A and B and C are true, Z must be true. You don't dispute THAT, I imagine?

T: If A and B and C are true, Z MUST be true. That's ANOTHER Hypothetical, isn't it? And, if I failed to see its truth, I might accept A and B and C, and STILL not accept Z, mightn't I?

A: You might, though such obtuseness would certainly be phenomenal. Still the event is POSSIBLE. So I must ask you to grant one more Hypothetical.

T: Very good. I'm quite willing to grant it, as soon as you've written it down. We will call it: (D) If A and B and C are true Z must be true.

A: Now that you accept A and B and C and D, OF COURSE you accept Z.

T: Do I?[4]

Now, as Achilles soon finds out, D is followed by E and E by F etc., *ad infinitum*. The moral of the tale: beyond a certain point, namely, the paradigm case, it makes no sense to ask for proof. You can't ask why the hypothetical in question is sound:

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[4] *Mind*, 1895; *Complete Works of Lewis Carroll*, Modern Library, pp. 1226, 1227-9.

the hypothetical is the "limit" of the proof; beyond it, there is nothing in terms of which it can be validated.

Similarly, the fundamental moral judgment cannot be validated. This does not mean that everything that follows from it is therefore unvalidated. It means only that in reaching the fundamental moral judgment we reach the terminating point of our validation. We cannot have it both ways: either there is an ultimate validating principle, i.e., a terminating moral judgment, or there is no validation. The cry for no ultimate judgment, i.e., for a reason for *every* moral judgment, *and* a validating procedure is irrational. It is like asking for the last member of an infinite series. We must reply: "There is no such animal; there can't be".

Moreover, moral theory is not alone in its reliance on the unproven. The difficulty would arise in natural science if we were to ask for a justification of induction (i.e., of a rule such as the following, which underlies scientific method: "We may argue from 'most instances of X's examined in a wide variety of conditions have been Y' to (probably) 'the next X to be encountered will be Y' ") before we made scientific predictions, on the ground that if the rule is unwarranted it makes no sense to base predictions on it. And in pure mathematics, Gödel's demonstration that there exist undecidable formulæ, i.e., formulæ constructive within a given system S, such that neither the formulæ nor their negations are demonstrable within S, might lead someone who insisted on determining beyond any possible doubt whether S is consistent to a difficulty of a similar sort. For Gödel demonstrates that another formal system, i.e., a meta-system S_1 could be employed to demonstrate the consistency of S. But suppose someone asks whether S_1 is consistent. If it is not, then any arbitrary formula could be deduced from its axioms, including the formula which says that S is consistent. So we should have to show that S_1 is consistent by using still another system, S^2, etc.[5] Paging Achilles and the Tortoise!

Thus, the demand in any field for a reason for an *ultimate* judgment, rule, or axiom is senseless. If there were a reason, the judgment, etc. would not be ultimate. It is not, then, a matter — to get back to ethics — of the fundamental moral judgment being "irrational" in any pejorative or commendatory sense of that term. We cannot call it "irrational" in any sense other than a definitional one. And when we see that this is so, we realize that we err logically if we apply the term "irrational" in a valuational sense to a *fundamental* moral judgment. And it follows at once that we cannot call an entire ethical system "irrational" in a valuational sense because its fundamental moral judgment is not supported by reasons. Nor can we, of course, call the *system* "irrational" in the descriptive sense, because if logically proper as opposed to merely causally influential reasons are offered in support of non-fundamental judgments in the manner suggested by Dr. Edwards, the system is rational, no matter what the epistemological status of the fundamental moral judgment.

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[5] Waismann, F., *Introduction to Mathematical Thinking*, New York, Ungar, 1951, pp. 100ff.

Elmont, New York.

II

By D. H. MONRO

Mr. Hoffman takes me to be making a general point about all reasoning; I was actually making a specific point about moral reasoning.

It is no doubt true that statements are validated by showing that they follow from other statements taken to be true. These may be validated in their turn; but obviously there must be at least one premise which cannot be validated in this way, but which must, it seems, just be taken for granted. Something else is taken for granted here too: the validity of the rules of inference which enable us to say that one statement "follows from" another. Mr. Hoffman does not distinguish between these two; but it is as well to notice that his example from Lewis Carroll, and the one about induction, apply to inference rules, whereas his main argument is about the validation of basic premises.

So far I have been talking about reasoning in general. But there is, I think, an important difference between moral reasoning and reasoning about matters of fact. When our reasoning is about a matter of fact, we shall, if we go on validating our premises long enough, eventually arrive at a basic premise that rests on the evidence of the senses: an observation-statement. With moral reasoning, on the other hand, the basic premise must be a statement which itself contains a moral term: a value judgment. And, whatever value judgments are, it seems clear that they are not observation-statements.

The problem of validation, then, may arise about any of these three: (1) rules of inference, inductive or deductive; (2) observation-statements; (3) value judgments. Mr. Hoffman assimilates all three, and takes me to be voicing a general, philosophic doubt which would apply indifferently to any of them. Now it is, of course, quite possible to raise philosophic doubts about both the rules of inference and the evidence of the senses. But these are not the doubts I was raising. I am quite happy (as a man if not always as a philosopher) to take both the evidence of my senses and the accepted rules of inference for granted. But I do feel uneasy about taking value judgments for granted. And I feel uneasy just because I am concerned with what Mr. Hoffman accuses me of ignoring: "the practicality of the moral situation".

It is quite true, as Mr. Hoffman says, that "we bring to a situation certain funded values which we do not doubt". But what if we are confronted with other people who do doubt them, and who have a set of values which *we* doubt? As I said specifically in my review of Edwards, this does not usually happen with induction. But it does happen with morals. Quite obviously, it happens to Dr. Edwards. Throughout his book he is anxious to defend a particular set of values against people in his own community who oppose them. He thinks that his values are rational and that theirs are not. As it happens, I am on Dr. Edwards' side in the practical controversy. But I do not see how he can combine this position with a meta-ethic which leads to the conclusion that, in Mr. Hoffman's words, "we err logically if we apply the term 'irrational' in a valuational sense to a fundamental moral judgment". Mr. Hoffman draws the comforting conclusion that we need not worry about our own value judgments being irrational. But of course it also follows that we cannot accuse our opponents of being irrational either. Perhaps it is just intolerant of me to be unhappy about this, but at any rate I do not see how Dr. Edwards can have it both ways.

This is of course an *argumentum ad hominem* against Edwards (though it happens to be the point I was making in the passage that Mr. Hoffman criticises); and it may not apply to Mr. Hoffman. But I find Mr. Hoffman's own position rather puzzling. In the main, he seems to think that any doubts about fundamental moral judgments are on a par with the doubts that Lewis Carroll might raise, as a kind of logical joke, about deduction; or perhaps with those that Descartes might raise, as a kind of exercise in doubting, about the senses. The assumption is that our "funded values" are at least as well grounded as observation-statements or the rules of inference; that there is no practical (as distinct from philosophical) reason for questioning them. It is odd to take this line in defence of Edwards; because it is (or is at least very like) the position of the intuitionists, whom Edwards attacks so bitterly. Intuitionists, too, are fond of comparing fundamental moral judgments with the rules of inference; they say, like Mr. Hoffman, that neither can be proved, but that neither need to be; they add (what he perhaps implies) that both are self-evident. And here one difficulty is that the difference (e.g.) between Edwards and his opponents (the puritans: in his more colourful language "the fuddy-duddies and the sourpusses") may well turn out to be a difference in their fundamental moral judgments. What is self-evident to him is very far from being so to them.

There is, however, at least a hint of a different position in Mr. Hoffman's article: that people do sometimes make different fundamental moral judgments, and that, when this happens, one judgment is as good as (or at least as "rational in a valuational sense") as the other. This is of course the subjectivist position, and it seems to be Dr. Edwards'. There are certainly strong arguments for it; my chief complaint against Edwards was that he did not seem to recognise its full implications. But I do not think that this position can be defended by an analogy with rules of inference or observation-statements. Would even the most inventive framer of deductive systems say seriously that any rule of inference was as rational as any other? Or that it is as rational to trust the authority of a sacred book as the evidence of the senses? We do not accept the evidence of the senses or the rules of inference just because we must start *somewhere,* and must have *some* rules of procedure.[1] We accept them because they work; and it is much easier to see what this means than to see what it means to say that a set of moral values works. This is, however, a large subject which I had better not try to pursue here. I hope I have said enough to suggest to Mr. Hoffman that the validation of fundamental moral judgments presents different problems from the validation of either the rules of inference or the basic premises of empirical arguments.

Sydney University.

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[1] See (by a pleasing coincidence) Mr. Goddard's article in this issue.

A NOTE ON QUESTIONS

By C. F. Presley

I wish to comment on two of the postulates put forward by Dr. Hamblin in his admirable discussion of the logic of questions.[1]

"*Postulate* 2. Knowing what counts as an answer is equivalent to knowing the question" (p. 162).

"Knowing what counts as an answer" might be taken to mean (1) "Being able to state all the possible answers to the question", or (2) "Knowing what form the answer will take". "Form" is used here in the sense in which "Luxembourg is in the continent of . . . " is the form of the possible answers to the question "In which continent is Luxembourg?" It should be made clear that if Postulate 2 is to be satisfactory, it must sometimes be interpreted in the one way, and sometimes in the other. This is because on interpretation (1) it is, in some cases, possible to know a question without knowing what counts as an answer, while on interpretation (2) it is possible to know what counts as an answer without knowing what the question is.

I may understand the question "In what state is Detroit?" (a), although I cannot list all the possible answers to this question because I do not know the names of all the United States. I also understand the question "What is the mean diameter of the earth at the equator?", although in this case I cannot state all the possible answers because there is no end to them. In both of these cases interpretation (2) is satisfactory: I know that the answer to the first question will be of the form "Detroit is in the state of . . . ", and that the answer to the second will be of the form "The mean diameter of the earth at the equator is . . . metres".

But there are cases where interpretation (2) will not do. Knowing that the answer to a question is of the form "John is . . . swimming", I do not know whether the question is "Is John still swimming?", or "Is John swimming already?" Here interpretation (1) is necessary: in order to know that the question is "Is John still swimming?" I need to know that the possible answers are "John is still swimming" and "John is no longer swimming".

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[1] Hamblin, C. L., "Questions", this *Journal, Vol.* 36, *No.* 3, (*December*, 1958), pp. 159-168.

The Detroit example (a) might suggest that in some cases one can know the possible answers without knowing the question. I am told that the possible answers to a question are "Detroit is in Ohio", "Detroit is in Vermont", "Detroit is in Texas", and so on, but I may not know that the question is (a) just because I do not know that the second name in each possible answer is the name of one of the United States. Nevertheless, from any given set of possible answers it is possible to construct a question by deriving a yes-or-no question from each possible answer and *joining* (see p. 166) all the questions so obtained: e.g., (b) "Is Detroit in Ohio, or is it in Vermont, or is it in Texas, . . . ?" If one accepts that (a) and (b) are different formulations of the same question — I take this to be Hamblin's view — then on interpretation (1) it is true that if one knows what counts as an answer then one knows the question. But it is only on interpretation (2) that knowing what counts as an answer follows from knowing the question. Philosophical questions give rise to special difficulties here.

"*Postulate* 3. The possible answers to a question are an exhaustive set of mutually exclusive possibilities "(p. 163). From this it follows that "If a question has only one possible answer, that answer is a tautology" (p. 164).

Consider the question "Can you hear me?" (c). In certain circumstances the only possible answer to (c) would be the contingent statement "I can hear you". But in unusual circumstances — where the person addressed is able and in a position to lip-read, for example — there will be two possible answers, and then, of course, the above theorem will not apply to (c). A question to which the theorem applies unconditionally, and of which it is false, is the less likely "Are you aware that I am trying to communicate with you?" (d). If the person addressed is aware of this attempt to communicate with him, then the answer is "Yes", but if he is not, then, even though by chance he were to say "I am not aware that you are trying to communicate with me", this statement would not be an answer because it would not have been made *in answer to* a question. So the only possible answer to (d) is "Yes", or more fully "I am aware that you are trying to communicate with me", which is not a tautology.

Associated with a question is an exhaustive set of mutually exclusive statements: let us call these "A-statements". According to Postulate 3, the A-statements associated with a question are the possible answers to that question. It is only if certain conditions are satisfied that a statement can function as an answer: let us call the statement of these conditions "C". If C is incompatible with one of the A-statements, then that A-statement is not a possible answer to the question; i.e., Postulate 3 breaks down. In such cases the disjunction of possible answers is not a tautology, and consequently even if such a question has only one possible answer, that answer will not be a tautology.

If (c) and (d) and others very like them are the only questions of this kind, it would be advisable to retain Postulate 3 and to note that (c) and (d) are not questions of the kind implicitly defined in Hamblin's system. This step is reasonable because (c) and (d) are more like call-signs than questions: they are ancillary to, rather than a part of communication, serving to make contact rather than to elicit information.

I have not discussed "Is this a question?", which is like (c) and (d) in some ways, or "Is your answer to this question true?", which is different, because they both differ from (c) and (d) in that we have no use for them.

Adelaide University.

MR. ARTHADEVA AND NAIVE REALISM

By D. M. ARMSTRONG

Mr. Arthadeva[1] is concerned with the difficulties that mirror-images raise for Naïve Realism. Many philosophers have argued in the following way:

1. Objects "in the mirror" are *seen* just as much as anything else in our field of view.

2. Objects "in the mirror" are not physical objects. On this basis they have concluded that:

3. The true objects of sight are not physical objects. They then wander in the wilderness prepared for them by Locke and Berkeley.

Mr. Arthadeva agrees with the first step in this argument, but at the same time he wants to deny its conclusion. As a Naïve Realist he wants to say that what we perceive is always something physical. He therefore argues:

1. Objects "in the mirror" are *seen* just as much as anything else in our field of view.

2. The true objects of sight are physical objects. On this basis he concludes that:

3. Objects "in the mirror" are physical objects.

Now this undoubtedly is a very tempting line of argument. Mirror-images do seem to be at least quasi-substantial. Many philosophers are therefore prepared to treat them as junior physical objects (Mr. Arthadeva points to the example of Professor J. L. Austin). Nevertheless, this line of argument has to face difficulties. Suppose I am two feet in front of a mirror. My mirror-image appears to be *two feet behind the surface of the glass*. Now the actual physical objects that occupy the space two feet behind the surface of the glass may well be such as to render it quite impossible that my mirror-double should be there too. (Suppose, e.g., the mirror is hanging on a wall which is more than two feet wide.)

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[1] This *Journal*, December, 1957.

Mr. Arthadeva offers an ingenious attempt to escape this difficulty. He suggests that when I look into a morror I am simply seeing *myself*. I am simply getting a view of *myself* that I am not normally able to get, *viz.* the view another person has when he faces me. And if I see myself directly and by means of the mirror at the same time, then I am simply in the fortunate position of getting two different views of the same object. Mr. Arthadeva is able to support his case by an interesting piece of linguistic evidence. We say "I saw myself in the mirror" and not "I saw an illusion", or "I saw another person who resembled me".

But there remains an important difficulty. Do not I and my mirror-image appear to be at *two different places?* And how can the same thing be at two different places at once? Mr. Arthadeva tries to get over this difficulty by arguing that the mirror-image does *not* seem to be in a different place from where I am. Place, he points out, is relative, and is determined by a thing's relations to other things:

> "Where, for instance, is my shoe? It is here (in front of the mirror): that is, between the glass before me and the wall behind, and between the chair on one side of me and the table on the other. It is *there* ('within the mirror'): that is, between the glass and the wall and between the chair and the table."

The description of "here" (in front of the mirror) and "there" ("within the mirror") are identical, therefore "there" is identical with "here".

CRITICISM OF THIS SOLUTION

Now I think we can say that when we look into a mirror what we see is ourselves. But if we do say so, we shall have to realize that we see ourselves *subject to certain distortions,* a distortion of place and the characteristic left-right distortion.

It is undoubtedly true that if we consider only the relations of an object A in front of the mirror to other objects also in front of the mirror, on the one hand, and the relations of the mirror-image of A to the mirror-images of the other objects, on the other hand, then there is no discernible difference between A and its mirror-image. But it is possible to show they are two in another way. If I stand two feet in front of the glass, then, as has already been pointed out, my mirror-image seems to be two feet behind the glass, and inverted, *which means that it seems to be four feet away from me, and inverted relative* *to me.* Now a thing that is four feet from me, to take only the first point, cannot be identical with me. Hence it must be admitted that when I look into a mirror there is at least an *element* of illusion. (This point tends to be obscured by the fact that we are very familiar with mirror-images, because of their usefulness in the conduct of life. But a child or dog, on first seeing a mirror, will think there is *another* child or dog *in front of itself.* This can happen to us, too, if we do not know there is a *mirror* in front of us.) Hence Mr. Arthadeva's solution fails.

AN ALTERNATIVE ACCOUNT.

I suggest that both the opponents of Naïve Realism, whom Mr. Arthadeva attacks, and Mr. Arthadeva himself, make the same mistake when they accept the premiss:

1. Objects "in the mirror" are *seen* just as much as anything else in our field of view.

For when objects seen by looking into a mirror are "seen" as "in the mirror" they seem to be behind the glass of the mirror. But there are no such physical objects behind the glass. Now I agree with Mr. Arthadeva that the only things we can see are physical things, hence I draw the conclusion that in such cases we do not see, but only *think* we see, an object four feet in front of us. And if it be replied that it seems to us to be seeing, the answer is that of course it *seems* so to us, for if it did not we would not think we were seeing. The cause of this false belief that we are seeing something is a matter for psychological investigation, and I have discussed the question elsewhere.[1]

This solution of the problem about mirror-images has to face one apparently very damaging objection. As already remarked, mirror-images deceive only the uninitiated, and the average person who turns his eyes towards a mirror does not believe that there is an object like himself behind the glass. How, then, can he think he is seeing such an object?

But when we look at mirrors it is *as if* we were looking at an object like ourselves but in front of ourselves, and it is only because we can bring all sorts of independent knowledge of the world to bear on the situation that we are not deceived. But for such independent knowledge it is logically necessary that we should be deceived. This suggests that we can say that when we turn our eyes towards a mirror we have a

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[1] This *Journal,* August, 1955, pp. 101-3.

temptation or *inclination* to believe falsely that we are seeing an object like ourselves but in front of ourselves.

In this way we can do justice to the element of illusion normally involved in looking at mirrors (an element which Mr. Arthadeva wrongly denies to be present), while at the same time avoiding the postulation of non-physical objects of vision. To speak harshly, there are no such things as mirror-images, there are only people who believe falsely or are inclined to believe falsely that they are seeing certain things that do not exist behind the surface of pieces of glass. The word 'mirror-image' has a use, but no substance corresponds to the substantive.

University of Melbourne.

CRITICAL NOTICE

THE MORAL POINT OF VIEW: A RATIONAL BASIS OF ETHICS. By Kurt Baier. Ithaca, Cornell University Press, 1958. xii, 326 p. $4.

Professor Baier writes clearly, unpretentiously and incisively; and he combines common sense with considerable analytic subtlety. His handling of the distinctions between related terms (such as "regulations", "mores", "principles" and "canons") is always acute and usually illuminating. He is admirable at breaking down a complex question (such as: "Is morality prior to society?") and showing the ambiguities that lurk within it. He can also put the essentials of a problem very neatly and clearly: his preface, which enumerates the "four main logical features of moral judgments" and points out that none of the traditional moral theories can account for them all, is a remarkably concise and penetrating statement of some of the central problems of moral philosophy.

Sometimes, it is true, one doubts whether his careful dissection of terms really does enable him to make the point he is trying to make; one may even suspect that the main thread of the argument has rather got lost during the patient examination of side issues. But the other side of this is that, even if one is dissatisfied with Baier's main argument, one can still find much of value in these incidental discussions. Since it is the main argument that I want to discuss, it is only fair to say this at the beginning.

What Baier is really trying to do, it seems to me, is to reconcile two very different, and at first sight incompatible, moral theories. One of these derives from Toulmin and, more remotely, from Bishop Butler: it might also be called "Ross without intuitions". The other is a modification of Hobbes. Since each of these theories stresses features of morality which the other ignores, the attempt to reconcile them is of great interest. I should perhaps make it clear that Baier does not himself explicitly advance these theories: in talking about them I am merely making explicit what seem to me — possibly not to him — the two main strands of thought in this book. The terms in which I state them are my own, which may not always be his; and, for ease of exposition, I am putting each theory in an extreme form, which he might want to modify.

Theory A: The difference between value judgments and empirical assertions has been much exaggerated. We test empirical assertions by applying the appropriate criteria: saying, for example, "This building is taller than that; it casts a longer shadow." We test moral assertions in the same way: saying, for example, "You ought to return the book; you promised to." Moral criteria can be questioned; but so can empirical criteria. The validity of empirical criteria depends in part on the meaning of words like "tall" and in part on empirical assertions, which can be tested in their turn, about such things as shadows. The validity of moral criteria depends on the meaning of moral terms and on empirical assertions about such things as human aspirations and emotions. The rule about keeping promises, for example, can be tested both by its consequences and by its fitting into a whole system of similar rules: the system as a whole constitutes "the moral point of view". The moral point of view can best be understood by describing it. Briefly, it is in Baier's words, the point of view of "an independent, impartial, objective, dispassionate, disinterested observer" (p. 201). Now, if anyone asks "But ought we to adopt that point of view?" the answer is that this is a meaningless question. "Ought" is itself a moral term, and to ask whether we ought to do something is to ask whether doing it accords with the moral point of view. If anyone asks: "Is a six-footer *really* taller than a man who stands four feet six?", the answer is: "Well, that's just what we mean by 'taller'". And, if anyone asks: "Is the moral point of view *really* better than any other?", the answer is "That's what we mean by 'better'". Or, as Baier puts it: "How could there be such a justification? It cannot be a justification in terms of self-interest, for it must be admitted that morality does not necessarily pay. Nor can it be in terms of moral reasons, for we are now talking *about*, not *within* morality. 'Why should I be moral?' cannot mean 'Does being moral pay?' or 'Is there a moral reason for being moral?' But it cannot mean anything else either, for there are no other types of reason. Hence it is an illegitimate question." (pp. 13-14.)

Theory B: Men live in societies, and their lives would be very miserable if they did not. If a society is to keep going, its members need to accept and, in the main, observe a set of rules about their behaviour to one another. It is these rules that we call 'morality'. Different societies may, up to a point, have different moralities; but there are some fundamental rules which should form part of the morality of any society. Otherwise the lives of its members will not be tolerable, and the society in question will be an immoral one. To ask whether an action is right is to ask whether it fits in with such a system of rules. The reason why the rules should be observed is that it is one's interest to live in a tolerable society.

Baier's position, as I understand it, is that theory A will do very well as an account of morality, from the point of view of morality itself. But the moral point of view does, he thinks, need the kind of justification that theory B can give it. We understand the moral point of view once we realise that it is forced on us by the needs of society; and we satisfy the needs of society because it pays us to. But he does not draw the conclusion that morality is ultimately justified by self-interest. What justifies both morality and self-interest is reason; and reason really is self-justifying, though morality is not.

Now, theory B does correct some of the defects of theory A. Philosophers nowadays often write as if moral principles were things, like trees or animals, which we just find lying about. They examine them carefully and report on their distinctive features (universalisability or whatever) much as biologists might report on the cellular structure of trees or animals. The biologist may, indeed, ask the further question: How does this animal come to have this structure? But there can be, for him, no question of *justifying* the structure. If anyone is misguided enough to ask: "But *ought* this animal to be like this?", he will shrug his shoulders and say: "Well, that is what it *is* like, and that is that". Similarly, many philosophers are inclined to say: "Moral rules just are universalisable, and the question 'Ought they to be?' just doesn't make sense."

Moral principles, however, are not like trees or animals. We don't just stumble across them. We make them. If we don't, as individuals, at least men collectively do. Following moral principles is a human activity. And we can't take human activities for granted, because presumably it is in our power to alter them, or to abstain from them, if we choose. If it is possible to order our lives by principles with a different set of formal features, someone sooner or later is going to ask the question: Shall we? And this is not the linguistic question: If we do, will the descriptive label "moral" still apply to our new principles? To say that we ought to follow a principle is not just to say that it has certain descriptive features (is "impartial", "disinterested", etc.). It is to claim that it should take precedence over other kinds of principle, when there is a conflict. And this claim does, on the face of it, need justification.

It is here that Hobbism (theory B) comes in, providing us with both an explanation and a justification of morality. If we ask: "Why are we moral?" then the answer is, as Baier notes, that "we have been trained to regard moral reasons as superior to all others, and we have accepted that" (p. 296). Men have trained their children in this way because they have come to realise that moral rules are essential to society, even the restricted society of the family. We can also understand most of the characteristic features of moral rules (universalisability, impartiality, and so on) once we realise that, to fulfil their social function, moral rules need to be teachable, publicly promulgated, and acceptable to everyone. And, if we ask why we *ought to be* moral, the answer is that morality is essential to society, and that we would be very unhappy without society. The ultimate justification, then, is self-interest.

But this is the snag. For, while it is no doubt to one's interest that other people should be moral, and worth being moral oneself to ensure that they are, occasions will arise when one can break a moral rule without immediate harm to oneself, and with no serious danger to the stability of society. Self-interest, then, even when enlightened, will sometimes conflict with morality. Or, as Baier puts it: "The best possible life *for everyone* is possible only by everyone's following the rules of morality, that is, rules which quite frequently may require individuals to make genuine sacrifices" (p. 315). It follows that "the moral point of view", the point of view which sets the best possible life for everyone above the best possible life for oneself, is not always justified by self-interest. Self-interest, then; cannot be the ultimate justification of morality.

It may be doubted whether this argument is as conclusive as it is generally thought to be. For, the consistent Hobbist might ask, why should one not exempt oneself from the moral rules on the rare occasions when no harm to oneself will result, either directly or indirectly? One can retort, of course, that it is not *right* to do so, or that doing so would outrage one's moral sense, but these assertions, though quite true, are beside the point. For "right", in this sense, means "in accordance with moral rules": in accordance, that is to say, with the rules which society promulgates for its own protection. And, of course, it could not be one of those rules that they need not be observed. As for the moral sense, that is the psychological result of one's childhood indoctrination in the moral rules themselves; so of course it is outraged by any proposal to break those rules. The only sense of "right" (or "ought") which is relevant here is quite a different one, in which it does make sense to ask whether it is right to break the moral rules. In this sense "right" means something like "rational". Now, if we ask whether it is rational to make exceptions in one's own favour (always assuming that one can do so without harmful consequences to oneself) the answer, on Hobbist principles, must be "yes". For, if it is rational to accept the rules of morality on the grounds that accepting them is the only condition on which there can be a society, and that society is essential to one's well-being, then it is rational to break those rules if one can break them without endangering the stability of society. The rational man will not, of course, advise others to break them even in these circumstances: for that would be against his own interests. But if he can break the rules in secret he will do so, while at the same time advising others always to keep the rules, in all circumstances.

Baier, however, is not prepared to follow Hobbism so far. He is quite clear that, when morality conflicts with self-interest, one ought to prefer morality. Yet he is not quite happy about the contention that it does not make sense to ask for a justification of morality itself. For moral reasons, he points out, are only one kind of reason for action. There are also reasons of self-interest: for example, that one enjoys doing something is a reason for doing it. ("Reason" here, it should be noted, means something like "good reason" or "justification": Baier distinguishes sharply between the causes of our actions and the reasons for them. The one word, he says, is explanatory: the other justificatory.) That another person enjoys your action is also a reason for doing it. But the first of these, Baier tells us, is a better reason than the second: there is no reason why you should sacrifice your own pleasure for the sake of an equal amount of pleasure for a complete stranger who has no claims on you. Finally, there are moral reasons; and these are better than either of the other two.

Now, "better" here can hardly mean "morally better", since the comparison is between moral reasons and other kinds of reason. Nor can it mean better from the point of view of self-interest, or of altruism. What then does it mean? Like the imaginary Hobbist I invoked above, Baier says that it means "more rational". It is "a rule of reason" that my enjoyment of an action is a reason for doing it. Moral reasons are also rules of reason. And it is a further rule of reason that moral reasons are superior to all others. The supremacy of morality over self-interest, then, has the same justification as has self-interest itself.

Morality, then, can be validated after all: by an appeal to reason. But how is reason to be validated? Now here, Baier says, we really do have an illegitimate demand. "Ought I to be moral?" is not a tautological question, since "ought" means, not "in accordance with morality", as has been thought, but "in accordance with reason". But it follows from this that "ought I to follow reason?" really is a tautological question. It is "as silly as 'Why is a circle a circle?'" (p. 317).

Now this is certainly more plausible than the parallel contention about morality, which it replaces. But can we be sure that the rules of morality are guaranteed by reason? Baier's argument here would certainly not satisfy the Hobbist. For what he does is to argue the case for saying that it is rational (other things being equal) to do what you enjoy doing. Then he says: "I need not waste any further time on examining whether the other consideration-making beliefs prevalent in our society are true. Everyone can conduct this investigation for himself" (p. 304). This is a very shaky foundation for the moral reasons and an even shakier one for the crucial contention that moral reasons are superior to reasons of self-interest.

What Baier actually appeals to when he invokes reason seems very little different from Ross's "intuition". Baier disputes this, but his argument turns on giving to "intuition" a meaning that Ross would never give it. He takes as the paradigm case of "intuition" the achievement of the mathematical prodigy who gives an instantaneous and correct answer to complicated arithmetical questions. But for Ross the paradigm case would be our grasp of the rules of inference. The propositions that, according to him, we "know by intuition" are those that are usually called "self-evident". Now, "that I enjoy something is a reason for doing it" would really do very well as a paradigm case of a self-evident proposition. Moreover, it might be contended that the refusal to accept a self-evident proposition is a paradigm case of "being irrational", "refusing to follow reason". I doubt if Baier's argument amounts to more than this. Anyone who seriously rejected the proposition "that I enjoy something is a reason for doing it" would, he says, be mad. But, if this is the test of our "consideration-making beliefs", will moral reasons pass it? People who reject moral reasons are not mad; they are immoral.

Baier has an additional argument for saying that it is a rule of reason that moral reasons are superior to reasons of self-interest. In considering this question, he says, "we are now looking at the world from the point of view of *anyone*. We are not examining particular alternative courses of action before this or that person; we are examining two alternative worlds, one in which moral reasons are always treated by everyone as superior to reasons of self-interest and one in which the reverse is the practice. And we can see that the first world is the better world, because we can see that the second world would be the sort which Hobbes describes as the state of nature" (p. 310).

But why is this the question we have to examine? Baier can say, of course, with considerable force, that the *moral* question is the one that he poses; for to take the point of view of morality just is to take the disinterested, impartial point of view: to prefer "the best possible life for everyone" to the best possible life for oneself. But the question here is whether that point of view is also the rational point of view. And Hobbes is no help here; for Hobbes' point is simply that, because he needs the co-operation of other men, the individual cannot hope to secure the best life for himself if it conflicts with the best life for everyone. Baier, however, disagrees with this. "A person", he says, "might do better for himself by following enlightened self-interest rather than morality. It is not possible, however, that *everyone* should do better for himself by following enlightened self-interest rather than morality. The best possible life *for everyone* is possible only by everyone's following the rules of morality, that is rules which quite frequently may require individuals to make genuine sacrifices" (pp. 314-5). It seems clear from this that, in Baier's view, it is rational to aim at the best possible life for everyone, not merely as a means to the best possible life for oneself, but even at the cost of forgoing the best life for oneself. Hobbes' argument does not establish this, nor would Hobbes himself accept it. And it is not enough simply to say, as Ross might, that we are all convinced that this assertion is true, that the contradictory belief outrages our moral consciousness. For, as I have mentioned above, the Hobbist can account for this: all that it means is that, in Baier's words, "we have been trained to regard moral reasons as superior to all others, and we have accepted that". But not every belief that we have been trained to accept is rational.

It is even possible that Baier is confusing two quite different things, either of which might be meant by "the point of view of anyone". In the passage quoted above from p. 310, in which that phrase is used, he is talking about principles which, if they apply to anyone, apply to everyone. In the passage from pp. 314-5, he is talking about the *interests* of everyone. Now, it is true that, if it is rational for me to put my own interests before the interests of everyone else, it must be rational for others to do the same. In that sense, the rational point of view is "the point of view of anyone". But it does not follow that it is "the point of view of anyone" in the other sense, the point of view of the general interest, or the best possible life for everyone, as distinct from the individual interest, or the best possible life for oneself. What does follow is that it is rational, not just for me, but for *anyone who can*, to secure his own interests at the expense of everyone else. Hobbes adds to this that in fact no-one can, because, if everyone tries to do this, no-one will succeed; and there is no way of stopping one's neighbours from trying, except by agreeing not to try oneself. The rational man will recognise this, and will consequently aim at "the best possible life for everyone", simply because this is the only practicable means of securing the best possible life for himself. But one cannot conclude from this, as Baier does, that, even if one could secure one's own interests at the expense of others, it would not be rational to do so.

My own feeling is that, although Baier plays off theory A against theory B, and theory B against theory A, he does not succeed in reconciling them. But one can be grateful to him for making the attempt: both because of the excellence of some of his incidental discussions, and because it is time that someone realised that Hobbism deserves a more convincing answer than it has yet been given.

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D. H. MONRO.

REVIEWS

English Philosophy Since 1900. By G. J. Warnock. (Home University Library). London, Oxford University Press, 1958. 180 p. 12*s.* 6*d.* (Australian).

Considered as what it is, rather than as what its title announces it to be, this is a book with considerable merits: it is an eminently clear, accurate and lively account of recent Oxford philosophy (pp. 94-179 are devoted to Ryle, Strawson, Waismann and Austin), and of such of its sources as are to be discovered in Moore (pp. 12-29), Russell (pp. 30-42), Logical Positivism (pp. 43-51) and Wittgenstein (pp. 62-93). For the rest there is a brief description (pp. 1-11) of Absolute Idealism (described as "The Point of Departure") and a number of "Retrospects", which are mainly devoted to a defence of contemporary Oxford philosophy against certain of its critics.

Mr. Warnock's book is certainly not, then, a history of English philosophy since 1900; his readers will not learn, either from text or bibliography, that pragmatism and realism ever occupied the attention of philosophers, nor that even such contemporarily-influential writers as Popper and Collingwood lived and wrote in England during the last fifty years. One cannot say, even, that Mr. Warnock is describing the emergence of recent philosophy at Oxford, for such a historical essay would have to pay considerable attention to Cook Wilson and his followers, who are here entirely ignored.

What Mr. Warnock has done, rather, is to describe the "big moments" in recent philosophy, the moments which would be selected for special attention by an intelligent but orthodox Oxford undergraduate preparing himself for an examination in philosophy. As a result, his book has a double interest: one can read it either for what it says about Oxford philosophers or as an expression of their historical attitudes — their attitudes, at least, to the recent past. Taken thus, Mr. Warnock's book must be warmly recommended as much the best introduction to contemporary Oxford philosophy, with a particularly useful emphasis on very recent work.

At the same time, it contains a number of *ex parte* statements about philosophy and its history which I should certainly wish to challenge; their general effect is to "play down" the extent to which philosophy has recently been following new paths. In this very "playing-down", of course, Mr. Warnock's work is representative of recent tendencies in Oxford philosophy, which now seeks to be accounted respectable rather than revolutionary, but he will certainly mislead the general reader.

The first such statement appears in the preface: "Most philosophy in English has been, for better or worse, and I shall not here say which, something vastly unlike most philosophy in other languages. This is not, as it is sometimes thought to be, a modern phenomenon. It has been broadly true for at least a century and a half, perhaps much longer." No doubt, one can so interpret Mr. Warnock's "most" that his statement is roughly true. But he is certainly slurring over the important fact that this tendency to isolationism has materially increased during the last fifty years. At the beginning of the century, Russell learnt much from Peano, Meinong, Frege, and wrote about Bergson; Moore's indebtedness to Brentano is obvious; Mach, Poincaré, Avenarius, Lotze, Renouvier were household words in philosophical circles. Even later, in the nineteen-thirties, Ryle was discussing Husserl and making a close study of Continental positivism. But now no influential English philosopher ever refers to his Continental colleagues — a fact which can only partly be explained by recalling that Hitler drove so many empiricists out of Europe. For it applies as much to French as to German philosophy.

Secondly, and more important, Warnock denies that there is any novelty in the tendency of recent philosophers to touch so little on the traditional "great issues" of morality, politics and religion. He goes so far as to assert that Hume's central philosophical ideas "bear on morality perhaps but on *religion not at all*" (p. 164, my italics), and makes the more general observation that the great philosophers of the past were not all concerned with religious questions, only sometimes with morality, and seldom with politics. The "great philosophers" he specifically mentions are Berkeley, Hume, Bradley, Locke, Hobbes, Descartes and Russell. Yet of these every single one except Descartes and Bradley published important works on religion, politics and ethics; and even Descartes and Bradley had something to say, even if this was not so considerable, on each of these topics. Perhaps one can justify the change that has come over philosophy and philosophers, but it is surely absurd to pretend that the change has not occurred; over the last thirty years philosophers have certainly been at once more insular and narrower in their interests than they have ever been before, explain that phenomenon how we will. And this fact, at least, ought to be made fully clear to the general reader.

JOHN PASSMORE.

WORDS AND IMAGES: A STUDY IN THEOLOGICAL DISCOURSE. By E. L. Mascall. London, Longmans, 1957. 126 p. 15*s.* (Australian).

This short work by the author of *He Who Is* and *Existence and Analogy* is clearly intended as a contribution to current Oxford discussion on religious language. As a prominent member of the group called Metaphysicals, composed of philosophers and theologians who have been working together on these topics, he has taken his full share of the burden, and he cannot be accused, as conservatives in philosophy are habitually accused, of "not understanding what is happening". Nevertheless, he sets out to defend the classical scholastic bastions against his environment. How far he allows himself to be infiltrated is another matter.

His overt criticisms of modern movements in philosophy are not particularly penetrating or original. On "theology and falsification" he is content to fall back on I. M. Crombie's contribution to *New Essays in Philosophical Theology* (reviewed in this *Journal* by J. A. Passmore, Vol. 35, No. 2, August, 1957). On R. B. Braithwaite's inaugural lecture he is firm and sensible, but, while properly insisting on the "obligatory factual affirmations" of any working religion, he does not see far enough into his opponent's position to develop its difficulties dialectically. *"Une religion sans Dieu! ...Mon Dieu, quelle religion!"* sufficiently summarizes his amazement. The real interest of this work is in the epistemological chapters II and IV, in which the modern error is traced to its source in the neglect of the scholastic distinction between *objectum quod* and *objectum quo.*

Epistemologists from Locke downwards have treated the sense-percept, through which we understand the world, as if it were the world to be understood: "Knowledge is of the agreement and disagreement between ideas." On that basis, if there is a world other than ideas, it is merely inferred from them, and, as Berkeley and Hume pointed out, the inference is both invalid and pointless. Following this line of succession we get Idealism (which killed metaphysics long before Schlick and Ayer) and Logical Positivism (which has solemnly fired its verbal bullets into the corpse). It is no use complaining about modernity unless one is prepared to go behind the 17th century. We do not know ideas; we know things *through* ideas.

With the idealist error goes another: the error of eliminating contemplation. In Aristotle or St. Thomas, contemplation is the crown of the intellectual life. Since the 17th century, reason has been reduced to ratiocination, losing its serenity and becoming a busybody; and has latterly been confined to the operation of rules which it has made for its own convenience. This, too, is the result of mistaking the *quo* for the *quod*.

Carrying his challenge to the source of the trouble, Mascall asserts: (i) that there is a non-sensory element in perception; (ii) that this non-sensory element consists not of inference but of apprehension; (iii) that the sense-object is to be sensed, not understood; what is to be understood is the "intelligible object of which it is a manifestation"; (iv) that, though the trans-sensory object is normally known through the sense-object, it is sometimes apprehended directly in mystical experience. Our summary comment on these propositions is: (i) that they are insufficiently argued (though the author might plead that he has argued them elsewhere); (ii) that they have somehow to escape from the difficulties of representative perception, which are not peculiar to the 17th century; and (iii) that some credit might have been given to at least one modern writer, John Cook Wilson, who in Mascall's own Oxford tried for years to formulate a logic of apprehension in opposition to the Idealist logic of judgment.

With the appeal from logic-chopping to contemplation the reviewer has every sympathy; but the sharp distinction which Mascall makes between trans-sensory cognition and knowledge by inference leads him into difficulties which more prudent Thomists have avoided: it is precisely on such inference that Thomists rely in their proofs of the existence of God. However, we find Mascall, reputedly an Anglican Thomist, borrowing from Don Illtyd Trethowan, a non-Thomist Catholic, his doctrine of "contuition" (p. 85). To contuit is "to apprehend the presence of the cause in a perceived effect": that is to say, proofs are not needed; in contuition cause and effect are present simultaneously. The purpose of the proofs is in that case not to win assent for a logical demonstration, but to put the hearer in the frame of mind in which the contuition shall occur. It would be interesting to have the comments on this move of an orthodox Thomist reviewer: it is to be suspected that he would respectfully raise his eyebrows.

Assuming, however, that there is "contuition" of sense-objects and their originals, or apprehension of the originals through sense-objects, the question arises: do we know the originals clearly and infallibly? Mascall's answer is, unequivocally, No. He is just as much opposed to "clear and distinct ideas" as to the confusion of idea and object. "My concern", he says (p. 82), "is to emphasize the essentially obscure nature of our apprehension of trans-sensible entities in the act of contemplative penetration." This sentence is written in connexion with our knowledge of God; but it is expressed in general terms, and may be taken to apply to trans-sensory knowledge of any kind. Contemplation, we are told elsewhere (p. 81) is not a seeing, but a wondering; "not a wondering *how,* or wondering *whether,* but wondering *at*". But this emphasis, while it does justice to the experience of the mystics, surely weakens the force of the word "knowing". We can no longer be said to know the thing by means of the percept; we merely peer into the hinterland of the percept with the aid of a fog-lamp. This is a conclusion which need not daunt the natural theologian, for he makes use of rational inference to see him through the fog to the summit. But it is awkward for a contuitionist, for all he can contuit with the percept is an obscuration.

It might seem that at that point faith must take over; and that would be a serious matter for Mascall, for, at that point, natural theology has not even started. He does his best in a tight spot by insisting that knowledge has nothing to do with clarity. He attacks the "dominant school of modern Anglo-Saxon philosophy" for its "over-anxious determination to avoid the possibility of error even at the cost of the quite ruthless impoverishment of the range of human knowledge" (p. 122), and he commends the exploration of the central mystery through a series of analogies (p. 97). But, if this is what knowledge amounts to, it is not directed to an "intelligible object of which the sense-percept is a manifestation", unless an intelligible object is defined as one which we can never understand.[1] And peering with a fog-lamp is not what is commonly meant by contemplation.

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[1] It occurs to me at the last moment that it is just this paradox which Mascall is trying to convey.

There is, no doubt, much to be said for a break between metaphysical inquiry and the sterile quest for certainty; and what Mascall has to say about the element of wonder and mystery in philosophy deserves a bearing in this age of "detachment, ratiocination, and attention to phenomena" (p. 73). But in that case it is misleading to talk about "intelligible objects" and still more misleading to talk about "contuition", for we cannot contuit two objects one of which is hidden from us.

Enough has been said to show that Mascall is, for a Neo-mediaeval thinker, unusually anti-rationalist, and the impression is confirmed in the final chapter on "Knowledge and Communication", in which he accepts, and enlarges upon, Austin Farrer's doctrine of the role of images in theological discourse. This is, briefly, that in the Bible, the source from which Christian theology flows, "the typical instrument of communication is not the concept but the image" (p. 109), and that "images cannot fulfil their proper epistemological function if they are approached in a merely rationalistic spirit" (p. 115). Not only so: "for the understanding of images it is not necessary to get behind them to a non-metaphorical understanding of fact" (p. 116). In theology, in fact, truth is communicated through images, which are drawn directly from the world of sense without conceptual intervention. This is as may be: personally I should have thought that the images used in theology were firmly anchored to affirmations about the state of the case. But, letting that pass, we are not provided with what Mascall seeks to afford us, "a rational justification for the activities of thinking and talking about God" (p. 121). We are provided with a philosophy in which reason is a faculty for assembling disparate and fugitive glimpses of what we at least cannot know, and a theology which cheerfully by-passes concepts altogether.

It is this discrepancy between the objective and the fulfilment which is disappointing, and it shows how difficult it is even for the most independently disposed to stand out against their intellectual environment, especially in a residential University. Mascall sets out with the best intention, of correcting a contemporary slant to linguisticism and phenomenalism, and makes some admirably pointed and pertinent comment upon it; but he gives way, at a higher level, to just the kind of theologico-philosophical thinking which linguisticists and phenomenalists can make terms with. It is all the more confusing (though also the more creditable) in that he does not want to make terms with them, and believes himself to have made a pertinent point against them. And so, in patches, as we have seen, he has.

But in identifying the intelligible with the obscure he has involved himself in a reverent agnosticism which supplements, without contradicting, modern Oxford philosophy, and from which only the authoritative definitions of his faith can bring him home.

It would be a mistake to give the impression that the book is unimportant. It is full of interesting suggestions, and, as an example of thinking against the current, most revealing. But it cannot have it both ways. Unless the *objectum quod* is somehow accessible to understanding, the accessory concept of contuition is of no avail. To introduce it as a substitute for the classical proofs, even under the guise of interpreting them, is to render it powerless. It may be that there are better ways of arguing the matter than those of Aristotle and St. Thomas — I think there are. But, given Mascall's chosen terms of reference, he has only one option: on to the full Thomist rationalism, or back to the fideism of his Christian colleagues in Oxford.

A. BOYCE GIBSON.

BOOKS RECEIVED

(Mention in this list neither precludes nor guarantees later review.)

ADLER, Alfred. The education of the individual. New York, Philosophical Library, 1958. 143 p. $3.50.

"The teacher, professional or amateur, must teach the simple thing: love, and call it by its simple name: love." — p. 114. He must also free the individual from "the classical concepts of space and time", and train him to "live his life as an eternal moment". The author is not the celebrated psychologist of the same name.

APFELBAUM, Bernard. Dimensions of transference in psychotherapy. (University of California Publications, Personality Assessment and Research, No. 2.) Berkeley and Los Angeles, University of California Press, 1958. 90 p. $1.75 Paper covers.

ARISTOTLE. On poetry and style. Translated, with an introduction, by G. M. A. Grube. New York, Liberal Arts Press, 1958. 80*c*. Paper covers.

ASOKA. Edicts; edited and translated by N. A. Nikam and Richard McKeon. University of Chicago Press, 1958. xxvii, 69 p. $1.75.

The moral doctrine of "Dharma", as proclaimed by the Indian emperor Asoka in the third century B.C.

BASSON, A. H. David Hume. London, Penguin Books, 1958. 183 p. 3*s*. 6*d*. (U.K.) Paper covers.

A critical exposition of Hume's views on epistemology, morals and religion.

BELL, Hermon F. Talks on religion. New York, Philosophical Library, 1958. 73 p. $3.

BERGSON, Henri. The world of dreams. New York, Philosophical Library, 1958. 58 p. $2.75.

A new translation, by Wade Baskin, of a lecture given before the Institut Psychologique in 1901.

COONEY, Timothy. Ultimate desires. New York, Philosophical Library, 1958. 100 p. $2.75.

"Our conclusion in this book is that the ultimate goal of life is Omniman . . . The ought of life is to continually advance the UPD (understanding-power-desire) curve." p. 95.

DRAKE, Henry L. The people's Plato. New York, Philosophical Library, 1958. xxiv, 633 p. $7.50.

A "complete reorganisation of the *Dialogues*" (in Jowett's translation) so that related passages are put together. For example, the section headed "Discovery of the Good Life" begins with pp. 27-38, 48-53, and 40-44 of the *Timaeus*, goes on with pp. 268-277 of the *Statesman*, pp. 245-256 of the *Phædrus*, and so on. The author gives his own comments in the manner of a *compère*.

FINDLAY, J. N. Hegel: a re-examination. London, Allen & Unwin Ltd., 1958. 372 p. 30*s.* (U.K.)
An interpretation which stresses Hegel's connexion with certain contemporary philosophical ideas and methods, but avoids argument with such other interpreters as Stace, Mure and Hyppolite.

FORDHAM, Michael. The objective psyche. London, Routledge and Kegan Paul, 1958. viii, 214 p. 25*s.* (U.K.)
Discusses Jung's theories and their application to psychiatry, anthropology and religious mysticism.

HANSON, N. R. Patterns of discovery; an inquiry into the conceptual foundations of Science. Cambridge University Press, 1958. ix, 241 p. 30*s.* (U.K.)
Examines the "philosophical aspects of microphysical thinking". The author holds that microphysics has often been misrepresented by philosophers because "they have regarded as paradigms of physical inquiry not unsettled, dynamic, research sciences like microphysics, but finished systems, planetary mechanics, optics, electromagnetism and classical thermodynamics" (Introduction). The discussion is therefore concerned with the logic of "growing research disciplines" and includes much of philosophical interest on such topics as observation, facts, causality and theories.

JOUVENEL, Bertrand de. Sovereignty; an inquiry into the political good. Translated by J. F. Huntington. Cambridge, University Press, 1957. xiv, 320 p. 27*s.* 6*d.* (U.K.)
"An attempt to supply a moral and philosophical basis for liberal institutions." Blurb.

KNELLER, George F. Existentialism and education. New York, Philosophical Library, 1958. xi, 170 p. $3.75.
Brief popular exposition of existentialism and its bearing on "modern" educational ideals, which the author regards as fighting against "the forces of depersonalization".

KOESTLER, Arthur. The sleepwalkers; a history of man's changing vision of the universe. London, Hutchinson, 1959. 624 p. 25*s.* (U.K.)
A history of cosmology, with emphasis on the relation between science and religion.

KUNITZ, Milton K. ed. A modern introduction to ethics: readings from classical and contemporary sources. Glencoe, Ill., Free Press, 1958. viii, 657 p. $7.50.
The extracts are grouped under eight main topics, ranging from "The Logic of Ethical Discourse" (Moore, Ayer, Stevenson, Ewing and Nowell-Smith) to "The Spiritual Life" (Zimmer, Niebuhr, Schopenhauer, and Santayana).

MELDEN, A. I., ed. Essays in moral philosophy. Seattle, University of Washington Press, 1958. xii, 216 p. $4.50.
Contributors are: Brandt ("Blameworthiness and Obligation"), Frankena ("Obligation and Motivation in Recent Moral Philsophy"), Hart ("Legal and Moral Obligation"), Hughes ("Moral Condemnation"), Prior ("Escapism: the Logical Basis of Ethics"), Ryle ("On Forgetting the Difference between Right and Wrong"), Singer ("Moral Rules and Principles"), Urmson ("Saints and Heroes").

Mill, John Stuart. Nature and Utility of Religion. Edited, with an introduction, by George Nakhnikian. New York, Liberal Arts Press, 1958. xxx, 80 p. 75*c*. Paper covers.

Mudry, Joseph. Philosophy of atomic physics. New York, Philosophical Library, 1958. 136 p. $3.75.
"In the presentation of this ideology, meticulous care has been exercised in avoiding the impression of a directed agnostication; but more conversely, a firm and vigilant approach has been sought to instil within the reader an awareness of two distinctions evolutionally transpiring in the affixation of the mind and matter interrelationships relative to their structure". Preface.

Pasch, Alan. Experience and the analytic: a reconsideration of empiricism. Chicago, University Press, 1958. xvii, 275 p. $6.50.
"The more rigorously and conscientiously empiricists have tried to work out their position the further that position has become removed from anything easily recognizable as experience. To examine and resolve this paradox is the aim of the following book." Preface.

Popper, Karl R. The logic of scientific discovery. London, Hutchinson, 1959. 480 p. 50*s*. (U.K.)
English translation of *Logik der Forschung,* with about 150 pages of new material, in the form of appendices.

Ritchie, A. D. Studies in the history and methods of the sciences. Edinburgh, University Press, 1958. vi, 230 p. 12*s*. 6*d*. (U.K.)
Chapters on the development of geometry, astronomy, chemistry, biology, and sociology, with the emphasis on the relation between facts and theory. "There is no one universal scientific method, any more than there is one universal method of catching fish. Each fish has to be caught by a method appropriate to that fish" (p. 7).

Reiss, Samuel. Language and psychology. New York, Philosophical Library, 1959. 299 p. $3.75.
Consists largely of lists of words intended to illustrate the different ways in which ideas become associated.

Schopenhauer, Arthur. The world as will and representation. Translated by E. F. J. Payne. 2 vols. Indian Hills, Falcon's Wing Press, 1958. xxxvi, 534 + 687 p. $17:50.
A new and, it is claimed, a more accurate translation.

Schrödinger, Erwin. Mind and Matter. Cambridge University Press, 1958. vii, 104 p. 13*s*. 6*d*. (U.K.)
The Tarner Lectures delivered at Trinity College, Cambridge, in October, 1956.

Solberg, Winton U., ed. The Federal Convention and the formation of the Union of the American States. New York, Liberal Arts Press, 1958. cxviii, 409 p. $1.75. Paper covers.
The documents, with a lengthy introduction.

Strauss, Leo. Thoughts on Machiavelli. Glencoe, Ill., Free Press, 1958. 348 p. $6.00.
Aims to show, by an examination of Machiavelli's peculiar rhetoric, that the "old-fashioned and simple opinion according to which Machiavelli was a teacher of evil" is "decisively superior to the prevailing sophisticated views", though not exhaustive.

TALLET, Jorge. The absolute being. New York, Philosophical Library, 1958. 74 p. $3.

VETTER, George B. Magic and religion: their psychological nature, origin and function. New York, Philosophical Library, 1958. 555 p. $6.

A discussion of the psychology of religion, by way of exposition and criticism of the most commonly held theories; and "a critical analysis of the chief metaphysical assumptions on which most religions rest, together with the evidence from contemporary science for abandoning them".

WOODWORTH, Hugh. Sanity, unheard of. Victoria, B. C., Sumas Publishing Co., 1958. 109 p. $3.

"The characteristic uneasiness of man is the inevitable outcome of his ability to think . . . Man's practice of thinking about his problems, far from resolving them, only settles him the more deeply in confusion and frustration . . . True sanity is an ecstasy, a constant joy, an exquisite state-of-being at present unknown to us because of the chronic tensions thought creates." Blurb.

WRIGHT, Chauncy. The philosophical writings of Chauncy Wright: representative selections; edited, with an introduction, by Edward H. Madden. New York, Liberal Arts Press, 1958. xxii, 145 p. 80*c*. Paper covers.

NOTES AND NEWS

A.A.P. ANNUAL CONGRESS, 1959

The Annual Congress for 1959 will be held at Adelaide from August 21st to August 26th. The Annual General Meeting will take place during the Congress. Accommodation will be available at St. Ann's College, Adelaide.

The Secretary of the Congress is Mr. C. F. Presley, Philosophy Department, The University of Adelaide, Adelaide, S.A.

JOHN ANDERSON TESTIMONIAL FUND

A fund has been opened to commemorate the work of Professor John Anderson on his retirement from the Challis Chair of Philosophy in the University of Sydney. Professor Anderson, who has several times been President of the Australasian Association of Philosophy, was Editor of this *Journal* for twelve years (1935-1946). He has made outstanding contributions not only to Philosophy but to the intellectual life of Australia.

The memorial is to take two forms:

(1) The publication, as a book, of Anderson's major philosophical articles;
(2) A portrait of Professor Anderson, to be painted by Mr. William Dobell and presented to the University.

Contributions are invited from readers of this *Journal,* which owes much to Professor Anderson.

Subscriptions should be addressed to "The Accountant, The University of Sydney, Sydney, N.S.W." and cheques etc., should be made payable to "The University of Sydney". All donations of £1 and upwards will be deductible for the purpose of Australian income tax.

NOTICE TO MEMBERS IN THE U.S.A.

Mr. Alan Donagan has kindly undertaken to act as the United States representative of the Australasian Association of Philosophy, membership of which is the cheapest way of subscribing to this *Journal.* Persons in the U.S.A. may join the Association through him. No money, however, should be sent to him.

His address is: Mr. Alan Donagan, College of Science, Literature, and the Arts, University of Minnesota, Minneapolis 14, U.S.A.

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